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Psychological Bulletin

MOTOKAWA'S STUDIES ON ELECTRIC EXCITATION OF THE HUMAN EYE[1]

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Much progress in visual science has been made by measuring the electric response of the eye to a photic stimulus. In this connection, one need only point to the work of Granit (**16**), Hartline (**18**), and many others. Less deeply explored is the possibility of revealing some of the mechanisms of vision by using an electric input to the eye and measuring the response in terms of the sensation of light such a stimulus evokes. The discovery that an electric stimulus will easily produce a sensation of bluish-white light was made by Le Roy (**30**) in 1755, and this effect has interested physiologists and psychologists sporadically ever since. Visual perceptions caused by inadequate stimuli are called phosphenes and it was suspected long ago that the electrical phosphenes might be altered to some extent by photic stimulation. The first systematic work on this problem appeared shortly after it became possible to control electric stimulation accurately. Electrical thresholds were measured at various levels of light adaptation and during the course of dark adaptation with conflicting results. Early investigators found no differences between the effects of light adaptation and dark adaptation (**8, 28, 68**), but later workers, such as Verrijp (**81**), Achelis and Merkulow (**2**), Bogoslovsky (**3**), and Schwarz (**75**), found the electrical threshold to be higher during dark adaptation. A new and perhaps very important turn has now been given to the problem of interaction between photic and electric stimulation by Prof. Kōiti Motokawa and his associates at the Physiological Laboratory of the Tohoku University, Sendai, Japan. These workers have been ingenious in their use of the threshold of an electrical phosphene as a means of analyzing the effect of photic stimulation on the activity of the visual mechanism. In a rather formidable output of some forty papers in the last four years, they have touched upon matters of brightness and color discrimination, color blindness, contrast, spatial summation, inhibition, flicker effects, visual illusions, and a new visual method of measuring fatigue. This is

[1] The writer expresses his appreciation to Prof. Kōiti Motokawa for his valuable comments on the original review, which appeared in the Minutes of the Twenty-ninth Meeting of the Armed Forces-NRC Vision Committee, 16–17 November, 1951. The present review closely follows the original and differs only in that the number of figures has been greatly reduced. This report was prepared under Contract NOrd 7386 between the U. S. Navy, Bureau of Ordnance, and The Johns Hopkins University.

work of very great interest and represents a completely fresh approach to problems in the field of vision. There is no doubt that it will be followed closely by visual scientists. However, it is much too early to characterize firmly these researches. A great deal of experimental labor must be done before this is possible. Since Motokawa's work is too recent to be yet well known, it is the purpose of this paper to give a preliminary review of the main results of his experiments.

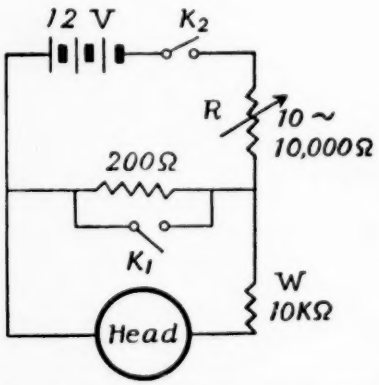

FIG. 1. GENERAL STIMULUS ARRANGEMENT USED BY MOTOKAWA. TIMERS, FREQUENCY GENERATORS, ETC. ARE CONNECTED INTO THIS OR SIMILAR CIRCUITS. K_1 AND K_2 ARE THE SWITCHES OF A SPRING RHEOTOME SET FOR DELIVERING A SINGLE PULSE.

METHOD

The electrostimulation method used by Motokawa is quite simple and is roughly the same for all his experiments. It differs in details from one study to another as research requirements change, but a statement of the general method here will suffice for most of the work.

Apparatus

The basic electrical equipment consists of a set of batteries, two flat electrodes, some kind of switch, a means of controlling the current passing through the tissues, and a method for measuring it. In addition to this, timers for producing pulses of variable duration, sine wave and rectangular pulse generators, and devices for delaying stimuli are used as required. Figure 1 shows the skeleton arrangement used. The auxiliary measuring, interrupting, and timing equipment connects into this basic circuit.

The optical devices used by Motokawa are conventional equipments, such as spectral light apparatus, filters, the Nagel adaptometer and anomoloscope, adapting screens, color tests, and similar well-known tools.

Procedure

The general procedure can also be broadly characterized for most of the work. The threshold of an electrical phosphene lends itself readily to measurement. It is, therefore, the threshold of this sensation that is used by Motokawa as an indicator of the effect produced by light stimulation. Two silver electrodes of about 2 cm. × 2 cm. are fastened to the body by electrode paste containing NaCl. These are generally applied to the glabella and the occiput, but sometimes the indifferent electrode is placed on the homolateral temple when only one eye is used. A weak, discrete stimulus current is then passed through the tissues and is increased or decreased in strength until a threshold phosphene is reported by the observer. The current must be interrupted, alternated, or given in pulses, since excitation occurs only on the make or break of the circuit. A continuous flow of current polarizes the tissues, and probably alters excitation, but does not stimulate the eye (cf. Granit [**15, 17**], Gernandt [**13**], and Kravkov and Golochkina [**26**]). Measurement of the threshold current is made by a suitably sensitive instrument across a series resistance that is high in comparison with the tissues. This method is thought to ensure a constant intensity of current through the tissues for a given applied voltage.

The photic stimuli that are used are presented in a great variety of ways: at different intensities, at different wave lengths, at different places on the retina, at different times in relation to electrical testing, at different repetition rates, in

different shapes, etc. The way the electrical threshold varies as the result of photic manipulation constitutes the bulk of the data presented by Motokawa and is the basis for his analysis.

Measurements

Data on the thresholds for electrically induced phosphenes are given in units of stimulus intensity required to evoke a just noticeable sensation. These may be expressed in either volts or amperes, depending on the choice of whether voltage or current is measured. Stimulus current is generally thought to be the more suitable measure, since variations of resistance due to changes in the tissues, sweating, and loosening of the electrodes are hard to control and may unduly affect voltage readings. It may perhaps be objected by some that Motokawa uses voltage. Nevertheless, he reports consistent measures, although in no case are enough data given to form an independent estimate of this.

In the absence of substantiation from other investigators, the main difficulty in a critical evaluation of the work is to determine the sufficiency of the evidence. Little appears in the published reports about the number of measurements taken at a given stimulus value and little is said about the number of *S*s measured. Curves and tables are usually presented as typical examples of the results. In some situations one needs a few measurements on a lot of *S*s; in others, a lot of measurements on a few cases. Motokawa's experiments, in general, would seem to demand the latter. Whatever the case, statistical evaluation of the evidence at hand is not possible.

Results

The results of Motokawa and his collaborators have been organized and presented under the following sections:

I. The excitability of the eye. This topic treats the effect of sensitization, adaptation level, time, position, and intensity.

II. Color discrimination. This includes the effect of wave length, intensity, color deficiency, color inhibition, and microstimulation.

III. Summation, contrast, and optical illusions.

IV. The stimulus strength-frequency relationship.

V. A new measurement of general fatigue.

I. *Excitability of the Eye*

There has long been uncertainty as to how the electrical excitability of the eye is related to the normal adequate stimulus. One of the most persistent problems has been the decision of where, in the visual apparatus, the electric stimulus acts. This is not the place to review the extensive material on this matter.[2] It will be sufficient merely to point out that a current passed through the eyeball is likely to be very diffuse in its effect and that the spread of stimulus current could reach anywhere in the retina or the optic nerve. However, if it can be shown that the presence or absence of light affects the electrical thresholds, some evidence for retinal involvement is indicated.

Sensitizing Currents

A few of the earlier studies bearing on the problem of adaptation have already been mentioned. The first paper that came from Motokawa's laboratory in 1948 attacked this matter. Motokawa, Iwama, and Endō (62) studied the effect of an intermittent, subliminal electric stimulus on the light threshold of the eye. They used 10 per cent subthreshold current pulses at 19 cy./sec. during the course of dark adaptation that followed 5 min. of pre-illumination to

[2] The writer is preparing an historical account of electrostimulation of the eye, running to two hundred titles, which will appear soon.

10,000 lux.[3] Adaptation was followed for 40 min. with a Nagel adaptometer. The results are shown in Fig. 2 for both a 12° area (A) and a 1° area (B) with central fixation. The solid curve is for threshold sensitivity to light as normally measured. The dotted curve shows the results when done: the electrical threshold for just noticeable flicker phosphenes at 14.3 cy./sec. was measured with, and without, threshold photic stimulation. Twenty-nine minutes of dark adaptation followed. The electrical thresholds continued to rise during the first 15 min. of dark adaptation

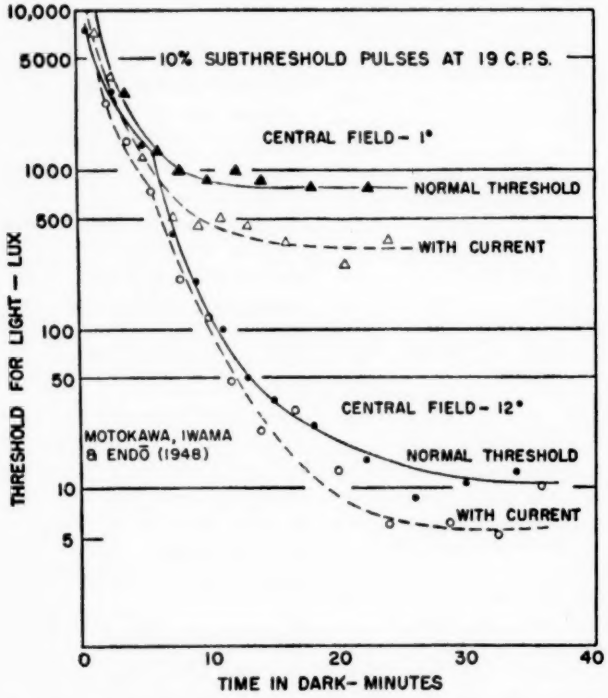

FIG. 2. EFFECT OF A SUBLIMINAL ELECTRIC STIMULUS ON THE PHOTIC THRESHOLD. [AFTER MOTOKAWA, IWAMA, AND ENDŌ (62).]

the current pulses were used. The presence of an electric current is shown in the data to produce an eye that is more sensitive to light. This appears true over both the rod and the cone portions of the dark-adaptation curve.

Sensitizing Light

The reverse experiment was also as measured both with and without light stimulation, and then leveled off. At all times, the electrical thresholds were lower in the presence of light.

This experiment clearly shows an interaction between electric and photic excitation processes and suggests that something gets sensitized by electric stimuli that makes it easier for light to act, and vice versa. However, it does not tell much about the site of electric stimulation. The

[3] The lux, or meter-candle, is equivalent to .0929 foot-candles. Ten thousand lux, therefore, are 929 foot-candles.

suggestion has frequently been made that the ganglionic cell layer of the retina is where the stimulus acts. Held to substantiate this is Iwama's finding that the eye's chronaxy, as as treated in the next section, is too large to be accounted for as that of the optic nerve, and that dark adaptation acts differently for electric and photic stimulation. Motokawa and Iwama (**54**) thought to come to closer grips with this problem by stimulating the eye with exponentially rising currents of various time constants in an effort to measure accommodation.[4] Three adaptation states were chosen: complete light adaptation to 10,000 lux, "moderate" light adaptation to a dimly illuminated white surface, and complete dark adaptation of about an hour. The presence of a threshold phosphene, as voltage was changed, was what was judged. The authors found that the value of Hill's time constant of accommodation (λ) was between 35 and 90 msec. for the completely light-adapted state, 17.5 and 25 msec. for the "moderate" condition, and 45 to 320 msec. after an hour in the dark. Bouman (**4**) states that this time factor decreases with light adaptation and is about 4 msec. in light and 40 msec. during dark adaptation. These results are not easy to interpret and one can say only that neural events in the eye as revealed electrically are altered in some complex way by the presence or absence of light.

[4] When a stimulating current is applied to a nerve tissue, excitability, after reaching a maximum value, tends to return to its initial or resting state. This effect is known as accommodation. If a current is increased very slowly, accommodation will act to prevent the threshold from ever being reached. Accommodation is complete when excitability returns to its resting value with the continued flow of a constant current. This occurs in some tissues. In others, it does not return at all. This latter is the case of no accommodation. Where there is no accommodation, a nerve will continue to give responses as long as the current flows, provided the intensity is of threshold level. The eye, incidentally, shows rather good accommodation as evidenced by the fact that a flash occurs only on the make and the break of the stimulus current.

Chronaxy

More evidence of the same kind of interaction was reported by Iwama (**23**) in a study in which he measured the time-intensity relationship of electrically exciting the eye under three different conditions of adaptation. These were somewhat as before: 950 lux for the light condition, about three lux for the moderate, and after 40 min. in the dark for dark adaptation. Average chronaxies for these three conditions were 6.2 msec., 30.8 msec., and 13.8 msec., respectively. Such large chronaxies are not characteristic of muscle and nerve tissues and are considered by Iwama to be too large to be attributed to the optic nerve. He is of the opinion that his values result from stimulating the ganglion cells of the retina. Iwama's chronaxies are much larger than those of 1 to 3 msec. measured by the Bourguignon school (**5**) but are in somewhat closer agreement with Verrijp (**81**).

Use of Pulses

Iwama also followed the change of electrical thresholds during the course of the first 25 min. of dark adaptation. He used rectangular pulses of different durations for this. Here it was found that the thresholds rose for a few minutes after the onset of dark adaptation and then reached a constant value at times that depended on the duration of the pulses. The shorter the pulses, the earlier the leveling off occurred. With 3.9-msec. pulses, no change in threshold

was observed after about 10 min. of dark adaptation. Stimulation with pulses of 52.5-msec. duration delayed this effect until about 20 min. In addition, Iwama stated that the chronaxy, during dark adaptation, passed through a maximum value at about 12 min. This again shows, in a general sort of way, that there exist, somewhere in the visual mechanism, a number of interacting factors that relate photic and electric excitation.

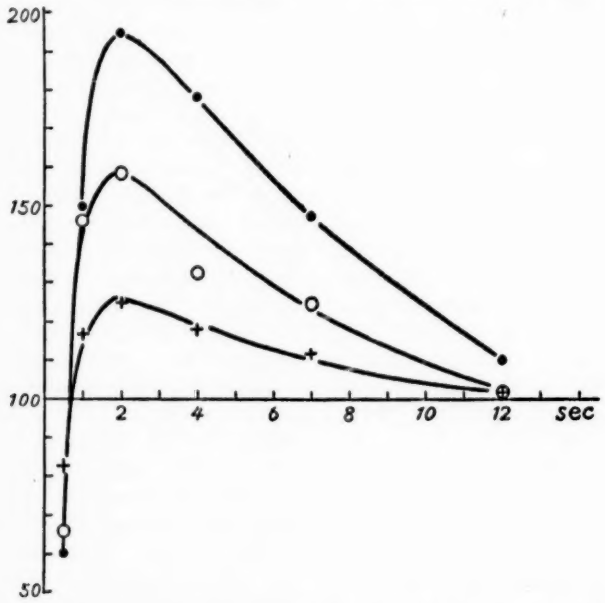

FIG. 3. E-CURVE SHOWING THE INCREASED ELECTRICAL EXCITABILITY OF THE EYE AFTER A SENSITIZING LIGHT. UPPER CURVE 70 LUX, MIDDLE CURVE 1.1 LUX, LOWER CURVE .06 LUX. ORDINATE: EXCITABILITY, PRE-ILLUMINATION VALUE TAKEN AS 100. [MOTOKAWA (39).]

Supernormal Excitability

Motokawa (**38**), in a detailed account of periodic excitability of nervous tissue, referred to a paper by himself and Iwama (**53**), in which it was demonstrated that a subnormal electric stimulus left in its wake an excitatory state in the retina that subsided like a damped oscillation.[5] When the reciprocals of the threshold intensities of electric test stimuli given at spaced intervals were plotted against the time after a subthreshold sensitizing current had been delivered, the result was an E or "excitation" curve. The amplitudes of the oscillations that were found depended upon the adaptation state of the eye and were noted to be largest for a moderately light-adapted eye. This finding led Motokawa to suspect that photic stimulation might also leave the same kind of excitatory state (**39**).

The investigation of this possi-

[5] Motokawa refers again to this effect in a later paper (**44**) where a sensitizing current that was 80 per cent of threshold was used. Oscillations at 18 cy./sec. were observed that die away in amplitude. Certain other complex frequency relationships relating to this phenomenon were also reported.

bility consisted in dark-adapting the eye for about 20 min. Then light exposures of from .2 to 2 sec. were given, followed by tests of the electrical threshold after a variable delay. The exposures were to white light at 70, 1.1, and .06 lux, and the thresholds were determined with constant current rectangular pulses of 57-msec. duration. The data

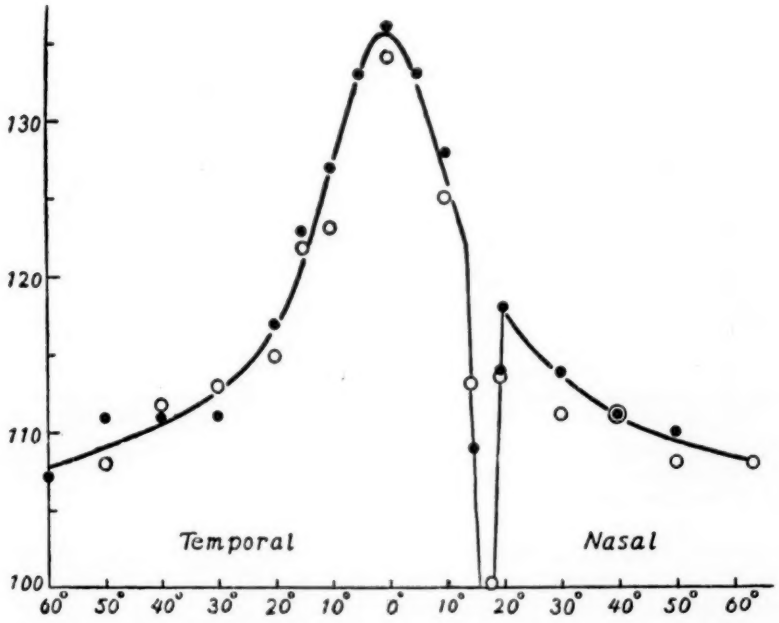

FIG. 4. EFFECT OF RETINAL POSITION ON INCREASED EXCITABILITY AFTER PRE-ILLUMINATION TO 287 LUX. ORDINATE: EXCITABILITY, PRE-ILLUMINATION VALUE TAKEN AS 100. [MOTOKAWA (39).]

are shown in Fig. 3, where the abscissa is elapsed time after pre-illumination and the ordinate is the reciprocal of the electrical threshold in percentage of what it was before illumination. The pre-illumination excitability value is taken as 100. It is seen from this curve that the eye becomes maximally excitable about 2 sec. after the cessation of white light. The time at which the crest in these curves was reached was independent of the intensity of the pre-illumination. It was also independent of the duration of the pre-illumination, at least at the fovea (**49**). If this test is made with a light stimulus, the sensitivity of the retina is, of course, always found to be lower. Motokawa again proposed that the probable locus of the electrical effect is the ganglion cells of the retina. He pointed out that the decay of an electrical sensitization process is very rapid (200 msec.), whereas the aftereffects of a photic process persist for 10 sec. or more. This difference he attributed to the involvement of photochemical reactions, and argued that if the optic pathways or higher centers were responsible, light and electricity, as

sensitizing agents, should behave in the same way.[6] He added further evidence of this by illuminating one eye only, but measuring the thresholds in both. The result showed that the enhanced excitability occurred only in the illuminated eye.

Retinal Position

It will be noted that Motokawa's method has by now taken on a distinctly analytical cast. In this same paper he tested further the versatility of electrostimulation as a research tool. He chose as his measure electrical thresholds taken at the crest point on the enhancement curve; that is, measured 2 sec. after the cessation of pre-illumination. He then pre-illuminated the retina with a 2° circular spot of 287 lux at different positions on the retina. Results for the left eye are shown in Fig. 4. Sensitivity is enhanced most at the fovea, is zero at the blind spot, and falls off markedly toward the periphery. Figure 5 shows the same measurements for electrical excitability following the use of a near-threshold intensity of pre-illumination. Curve A (solid) is the electrical data. Curve B (broken) is sensitivity to light as measured with an adaptometer. The curves agree remarkably well.

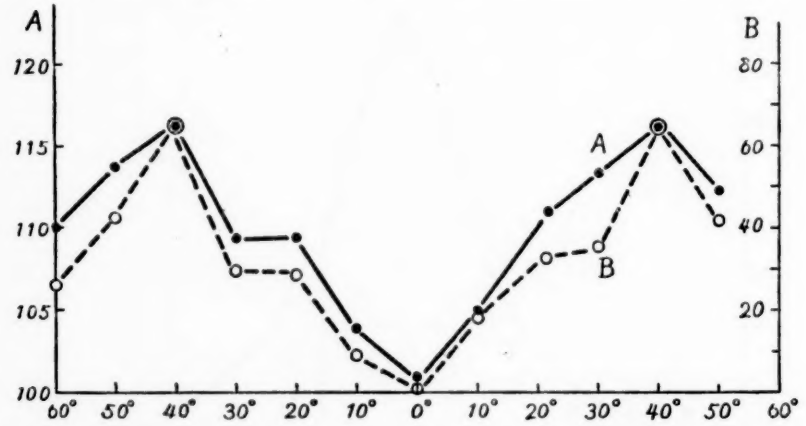

FIG. 5. (A) EFFECT OF RETINAL POSITION ON EXCITABILITY AFTER PRE-ILLUMINATION AT A NEAR-THRESHOLD INTENSITY. (B) SENSITIVITY TO LIGHT AS MEASURED WITH AN ADAPTOMETER. [MOTOKAWA (39).]

Intensity of Pre-Illumination

The next step was to determine the effect of the intensity of the pre-illumination on the electrical excitability of the eye (**59**). The test current was confined to the illuminated eye by using electrodes above the eye and at the outer angle. The test pulses were 100 msec. in length. The size of the pre-illuminating test patch was 2° for observer K.I. and 8° for K.M. It was centrally fixated. The measure ζ plotted against log I for two *O*s is given in Fig. 6. A word of clarification is in order here,

[6] Another difference between electric and light stimulation is that flickering phsophenes can be experienced to frequencies of intermittent current up to 80 cy./sec. or more (**31**). This is far above the fusion frequency for light and appears to be due to the absence of the chemical reactions that must operate in the case of light.

as this symbol will be used frequently in the discussion to follow. The measure ζ is defined as

$$\zeta = \frac{E - E_0}{E_0} \times 100,$$

where E_0 is the excitability, or reciprocal of electrical threshold, as shown by Iwama to be constant by then (**23**). We may now return to Fig. 6 in which the second curve is displaced to the right two log units. The dotted curve is attributed to the rod function since the 8° area covered more than the fovea in K.M.'s case.

It was pointed out that the data obtained in this experiment bore a

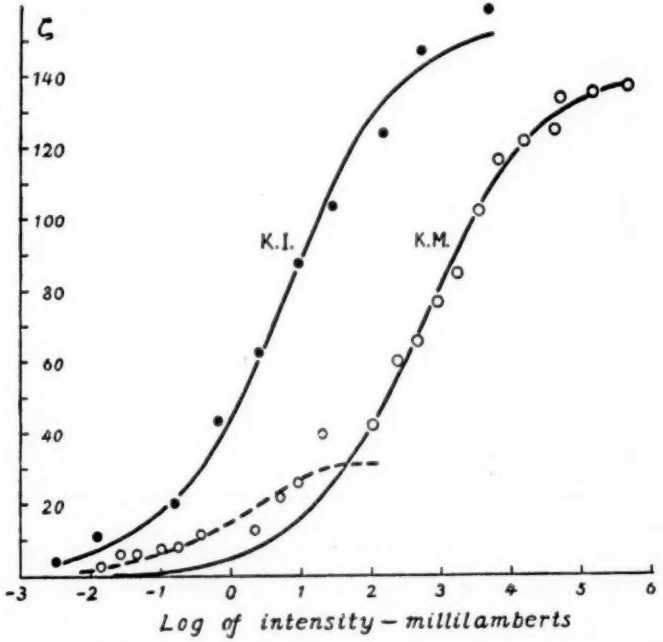

FIG. 6. EFFECT OF INTENSITY OF PRE-ILLUMINATION ON EXCITABILITY. ORDINATE: ζ IS THE PER CENT INCREASE IN ELECTRICAL EXCITABILITY DUE TO LIGHT OVER WHAT IT WAS WHEN MEASURED IN THE DARK. [MOTOKAWA AND IWAMA (**59**).]

determined in the dark after 20 min. of dark adaptation, and E is excitability as determined after pre-illumination has ceased. The quantity ζ, therefore, is the increase in electrical excitability due to light over the electrical excitability in the dark expressed as a percentage. Twenty minutes of dark adaptation were chosen for the base E_0, inasmuch as the threshold to electric pulses was marked similarity to those obtained when CFF and visual acuity are plotted against light. Hecht (**20**) had shown that certain visual phenomena could be reasonably well described by the equation

$$KI = x^n/(a - x)^m,$$

where I is the light intensity, x the concentration of the photolytic substance in the stationary state, a is

the initial concentration of the photosensitive substance, K expresses the ratio of the velocity constants of the light and dark reaction, and m and n are numbers characterizing the order of the light and dark reactions, respectively. Motokawa proceeded to relate his measure ζ to this formula by substituting it for x, and the fovea the measures were taken. Figure 7 shows the results of measurements taken with a 2° pre-illuminating patch at the fovea, and at 5°, 20°, and 50° removed from it. Motokawa concluded that the slowly rising portion of these peripheral measures for low intensities represented rod function, and the steeper

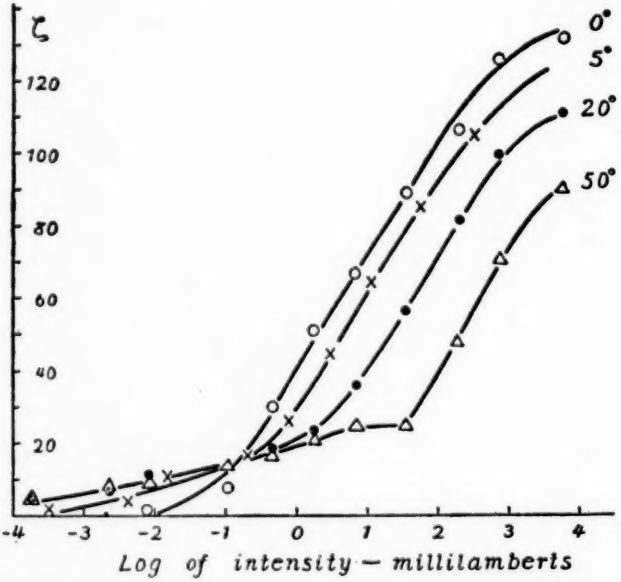

FIG. 7. ζ-LOG I CURVES FOR PRE-ILLUMINATION AT DIFFERENT RETINAL POSITIONS. [MOTOKAWA AND IWAMA (59).]

solid curves shown in Fig. 6 were computed by this means, m and n both being taken as 2 as in Hecht's case. Motokawa had suggested in the previous paper (**39**) that a chemical or photosensitive substance was involved that raised electrical excitability as a consequence of light action. The success of using Hecht's equation, he felt, added convincing evidence for it.

Outside the fovea, the sigmoid relationship between ζ and intensity consisted of two segments that became more distinct the farther from portions at high intensities were due to the cones. In any case, satisfactory descriptions of this and the preceding data were obtained by substituting ζ in Hecht's equation to obtain

$$KI = \zeta^2/(a - \zeta)^2.$$

Instantaneous Thresholds

The work on the time-course of excitability following pre-illumination so far reported had not included measurements at delays shorter than

about one-quarter of a second. Since it was indicated that a short subnormal excitability period existed before the rather lengthy supernormal period, Mita, Hironaka, and Kōike (**33**) investigated the region between the time when pre-illumination ceased and 2 sec. Except for more elaborate timing equipment for controlling the duration of pre-illumination exposure and the introduction of the test stimuli, the method was as already described. A 1.5° circular field of light and 140-msec. test pulses were used. In studying the effect of intensity for light exposures of 1/250 sec., it was first noted that at all intensities the value of ζ was negative (subnormal excitability) for about .75 sec. It then rose to a maximum at about 2 sec., as had previously been shown. In addition, the finding was verified that the maximum value reached by ζ increased with the intensity of pre-illumination. During the subnormal period, however, the reverse occurred. The lowest values of ζ were reached when the highest intensities were used. Another aspect of this study showed what happened when intensity was held constant and the exposure time was varied. The highest supernormal values of ζ were reached with long exposures. The results for the subnormal period are confusing, although there is an indication that the length of the subnormal period may be inversely related to the length of the light exposure.

"*Empfindungszeit*"

Bearing upon another aspect of the general problem of the excitability of the retina is a rather uncertain paper by Hironaka (**22**). He pointed out that the problem of where the electric stimulus acts in the eye is still unsolved and suggested the comparison of the *Empfindungszeit* for a photic and electrically induced sensation as a means of settling the matter. The *Empfindungszeit* (EZ) was proposed by Fröhlich as a term for the time required by a stimulus to evoke a sensation (**10**). The photic EZ had been measured by Mita, Hironaka, and Kōike (**34**) for different conditions of adaptation and different retinal positions in a previous contribution. Hironaka employed the same method for the phosphenes and reported that EZ's for light at the fovea varied from 67.5 to 185 msec. depending on the light intensity, and for phosphenes in the dark-adapted eye from 68.5 to 176.8 msec. depending on the current. These agree well. For EZ's taken 50° out on the periphery, however, the values were about 358 msec. for light. Since this is about twice the EZ for the phosphenes, Hironaka related the foveal light EZ with the effect of electric stimulation and assumed that the electrical excitability and speed of reaction of the cones were higher than that of the rods.

It is not completely clear that this is the case, although there is evidence that the scotopic process is slower than the photopic. The fusion frequency for light is lower in the peripheral retina than in the central region, and dark adaptation proceeds slower in the rods than in the cones. In addition, Motokawa and Ebe (**49**) have presented data to show that the rod process, as determined by the ζ-time method, is the slowest in the retina. On the other hand, work by Bourguignon and Déjean (**5**) and Kleitman and Piéron (**25**) shows the speed of response of the cones to be less than that of the rods, whether measured by light or electric stimuli. Whatever the case, the problem of measuring the speed with which sensations and perceptions are

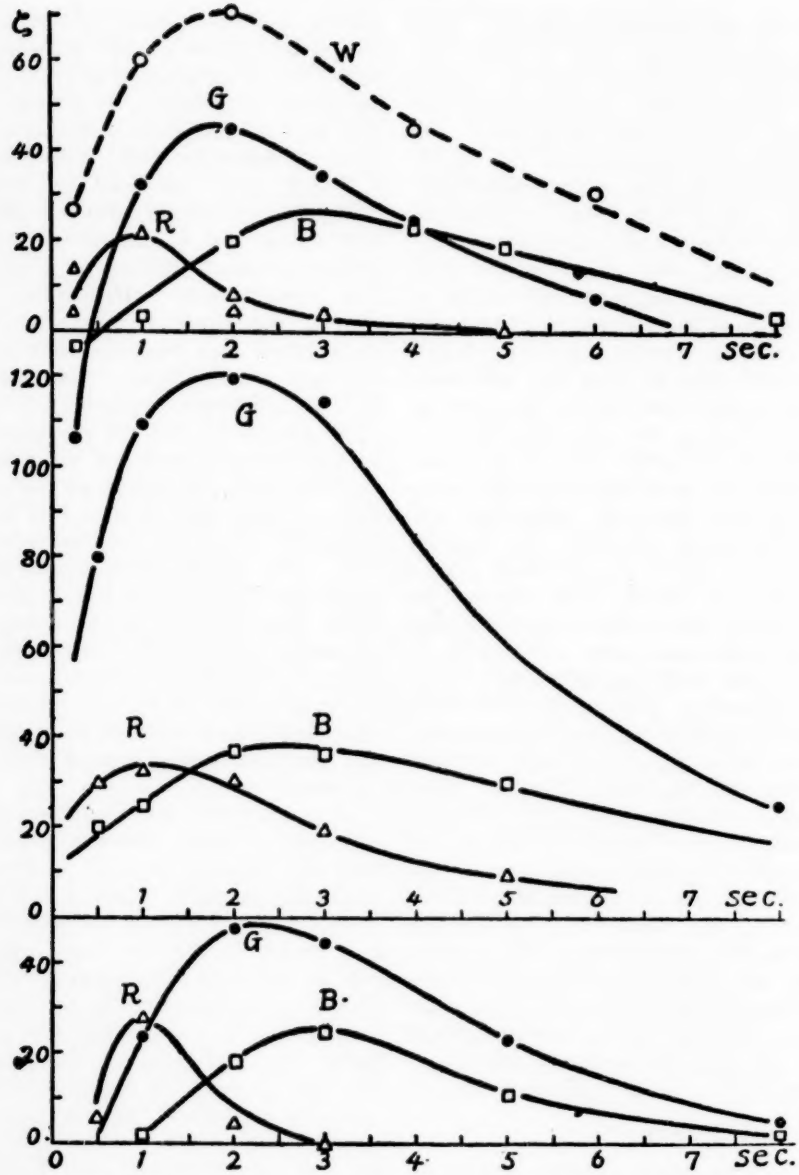

FIG. 8. ζ-TIME CURVES FOR THE NORMAL EYE WITH RED, GREEN AND BLUE LIGHTS OBTAINED BY FILTERS. THREE DIFFERENT SUBJECTS. [MOTOKAWA (40).]

aroused has always been a knotty one and it is not surprising that agreement has not yet been reached.

II. *Color Discrimination*

The results of the physiological aftereffects of brief illumination with

white light suggested, quite naturally, that a similar examination be conducted with colored lights. The time-course curve of ζ that showed the retina to be at maximum excitability 2 sec. after the termination of neutral pre-illumination might well be of different shape with selective stimuli. The method used to investigate this aspect of the problem was the same, in the important details, as has been described for the white light studies.

Hue and the Time-Course of Electrical Excitability

In the first study of this kind reported by Motokawa (**40**), both filtered light and monochromatic light from a dispersion spectrum were used. The pre-illumination was a spot of 2° exposed for .2 sec. and the electrical testing was done with 100-msec. pulses. Samples of filtered light data for three different subjects are shown in Fig. 8. The filters used were red (620–740 mμ), green (490–550 mμ), and blue (410–490 mμ). It is to be noted that the sets of curves differ markedly in shape. This matter of striking individual differences suggests color anomalies in vision, a problem to be taken up in the next section. But of primary interest here is the fact that the maxima of the red, green, and blue curves are reached at different times; namely, at about 1, 2, and 3 sec., respectively. The white curve, shown in the upper part of the figure, corresponds to the green maximum except for amplitude. When Motokawa compared the foveal and peripheral regions by pre-illuminating with filtered lights, he found that the ζ-time curves taken at 40° out on the periphery were very nearly the same for all hues and that in each case the maximum occurred at 2 sec. The relevance here for color theory is obvious.

Effect of Intensity

In this same paper was shown the effect on ζ of reducing the intensity of spectral lights of 650, 585, 530, and 470 mμ. The published curves indicate that the maximum value of ζ for any given wave length underwent no shift in time as the intensity was reduced. It seemed to be a property of the wave length alone. The yellow light at 585 mμ afforded a slightly later maximum than the red and was located between the red and green at about 1.25 sec. to 1.50 sec.

Motokawa emphasized in this paper that time in these curves concerns the rate of the recovery process of the retinal physiological mechanism after illumination: the longer the wave length, the more rapid the recovery. Intensity concerns the magnitude of physiological process but not its rate. Time and intensity, therefore, are shown by the data, to be independent.

Wave Length and the Maxima of the ζ-Time Curves

The times after pre-illumination at which the maxima of the ζ measure were reached were investigated by Motokawa (**42**) for spectral light of different wave lengths. It was found that the time, τ, at which the maxima occurred, varies with wave length, λ, as shown in the upper curve of Fig. 9. Time, τ, varies between something over 3 sec. for light of 400 mμ and about 1 sec. for light of 700 mμ. The curve connecting these two extremes is not smooth and τ changes rapidly with respect to λ in several places. The next curve shown in Fig. 9 is for $\Delta\lambda$ and was obtained by conventional methods of measuring hue discrimination. The data are for Motokawa's eye and show minima at 460, 490, 585, and 630 mμ. Final-

ly, the curve $\Delta\lambda/\Delta\tau$ was obtained by plotting the decrease of wave length $\Delta\lambda$ corresponding to the reciprocals of the slope of the τ curve against λ. This curve corresponds reasonably well to the hue discrimination function and is taken to be further evidence that a basic physiological process is being tapped by the electrical method. Once more it was asserted that this mechanism is peripheral and lies in the ganglion cell layer of the retina.

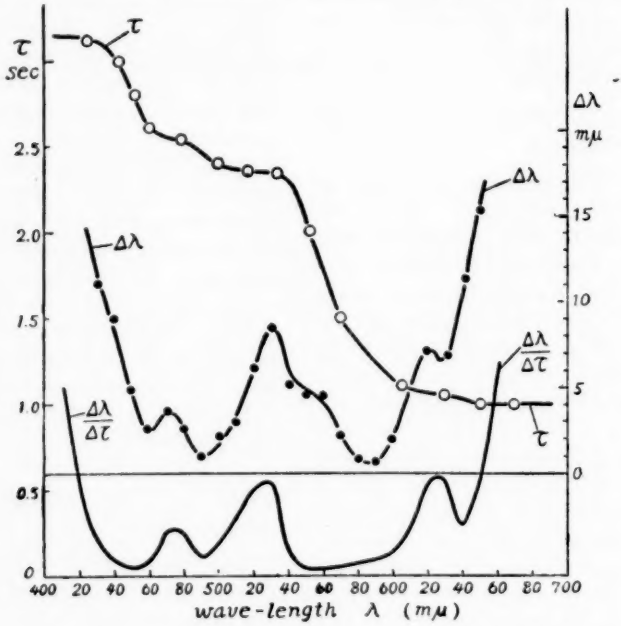

FIG. 9. UPPER CURVE: THE TIME (τ) AT WHICH THE MAXIMUM FOR ζ OCCURS FOR EACH WAVE LENGTH (λ). MIDDLE CURVE: CONVENTIONAL HUE-DISCRIMINATION. LOWER CURVE: THE CHANGE IN WAVE LENGTH ($\Delta\lambda$) CORRESPONDING TO THE RECIPROCALS OF THE SLOPE OF τ FOR EACH WAVE LENGTH (λ). [MOTOKAWA (**42**).]

Color Deficiency Studies

The foregoing success in isolating distinctive patterns of electrical thresholds for selective illumination of the retina next led to the important question: What can be learned from cases of color blindness? The method used to examine persons of deficient color vision was the same as has been described for preceding studies on the determination of the ζ-time curves. Figure 10 from Motokawa (**43**) is the excitability curve of a deuteranomalous subject measured after pre-illuminating the center of the eye with white light. Three maxima appear at times corresponding to those obtained by applying red, green, and blue lights separately to a normal eye. A normal trichromat, it will be remembered, manifests only one maximum to white light. Motokawa noted that for the deuteranomalous case the more weakly excitable G curve dropped out rapidly as intensity was reduced. He felt that he had here basic physiological evidence of the three independent processes assumed by the three-components theory of color

vision. More evidence of this same kind is given by data presented in later papers by Motokawa (**35**, **36**), Motokawa and Suzuki (**65**), and Ebe, Isobe, and Motokawa (**9**), where the method was extended to measurements on deuteranopes and protanopes, as well as normal eyes measured when pre-illumination was applied outside the fovea. Figure 11 from Motokawa (**35**) shows a further analysis of what happens in the deuteranomalous eye and compares it with normal color vision. For the normal eye, light of different wave lengths produces maxima in the ζ-time curve that are progressively displaced forward in time as wave length is shortened. This fact has been discussed in the preceding section (**42**). Motokawa assumes here that the continuous ζ-time curve of white light is a composite of the individual curves that result from selective stimulation. For the deuteranomalous case, however, the resulting family of curves reveals excitability maxima that at all times reflect the weakness of the underlying physiological G process. For white light these sensitivities fuse into a composite ζ-time curve with three maxima.

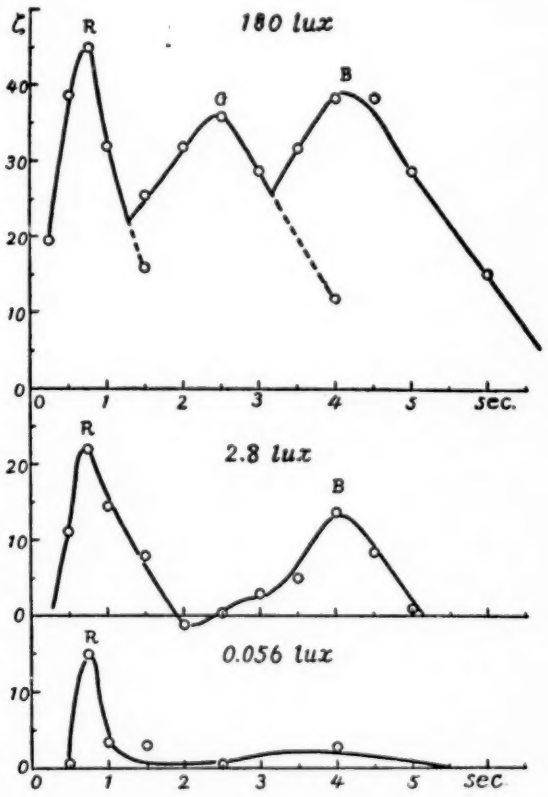

FIG. 10. ζ-TIME CURVES FOR THE DEUTERANOMALOUS EYE OBTAINED WITH WHITE LIGHT. [MOTOKAWA (**43**).]

To show the effect of retinal posi-

tion, Motokawa stimulated the normal eye extrafoveally at 5°, 10°, 15°, 25°, 35°, 45° and 50°. The pre-illuminating stimulus was a 2° patch of white light. The results are shown in Fig. 12. At 5° and 10° a curve of three maxima is seen. This is taken to be indicative of deuteranomaly in the parafovea of the normal eye. Further out on the periphery, the R and G maxima both drop out, leaving those corresponding to the Y and B. Up to this point the electrical findings correlate with what is found in ordinary perimetric studies. At 50° a ζ-time curve results showing a single maximum at 2 sec. The nature of this residual excitation curve is thought of as a rod mechanism.

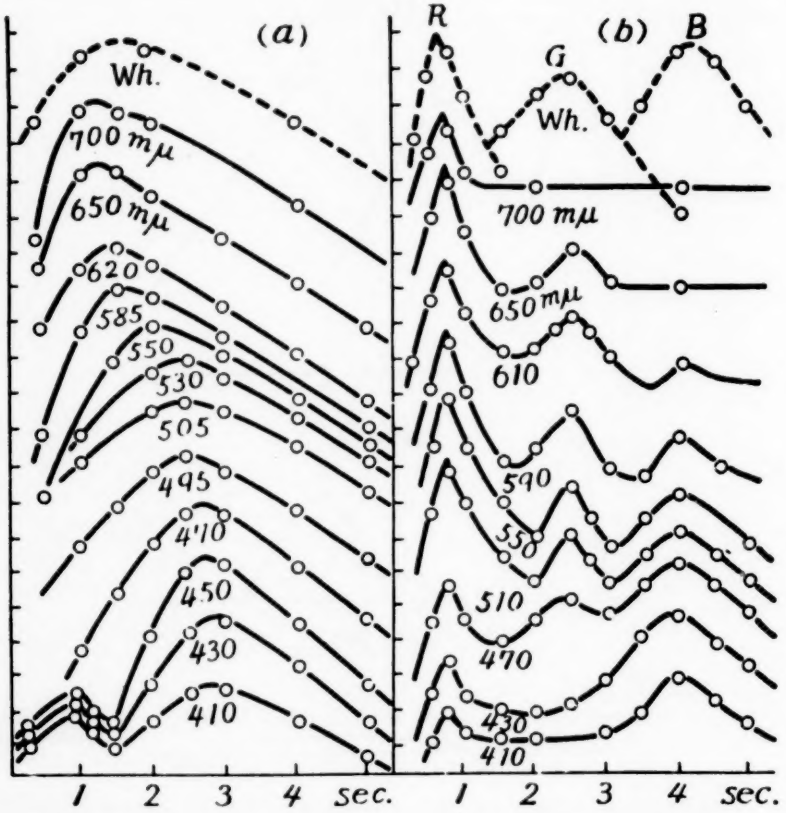

FIG. 11. EFFECT OF WAVE LENGTH ON THE ζ-TIME CURVES OF NORMAL (a) AND DEUTERANOMALOUS (b) EYES. [MOTOKAWA (35).]

Physiological Sensation Curves

In a further attempt to isolate the nature of the basic physiological processes in vision by electrostimulation, Motokawa (**35**) performed a very interesting experiment. He had previously determined the times after pre-illumination at which the maximum of ζ occurred as a function of wave length. Now he explored wave length, using the three times at which the maximum to red, yellow, green,

and blue light occurred. These were taken as 1, 1.5, 2, and 3 sec., respectively. The upper set of curves in Fig. 13 is due to illuminating the fovea of the normal eye and shows how ζ varies with wave length when measured at these times. Motokawa and 4 sec. for B. The results for each wave length may be seen by comparing Fig. 13 and 14 for the trichromat and deuteranomalous cases, respectively.

First, it is seen in Fig. 13 that for the normal subject the B process

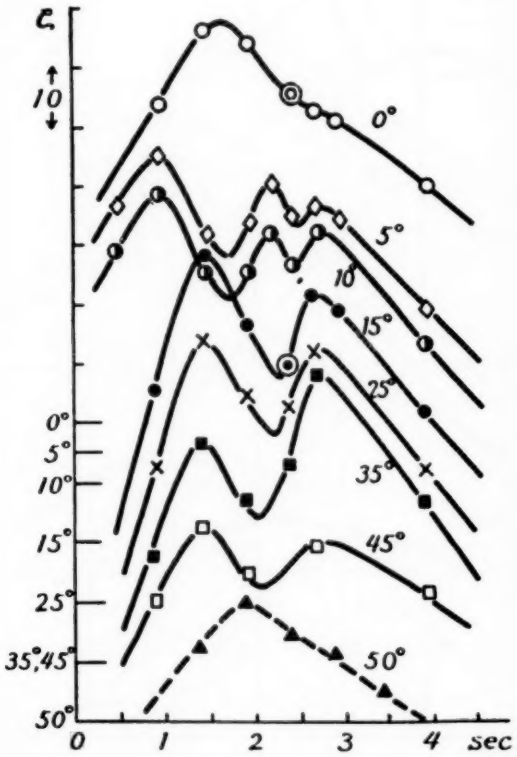

FIG. 12. EFFECT OF RETINAL POSITION ON THE ζ-TIME CURVE OF THE NORMAL EYE. [MOTOKAWA (35).]

calls these functions physiological sensation curves and points out their striking similarity to those classically obtained by color mixture. He then proceeded to use this method of analysis on both normal and deuteranomalous eyes at different positions on the retina: 0° (fovea), 15°, 25°, and 35° (36). For the color-deficient case the time values were: .75 sec. for R, 1.5 sec. for Y, 2.5 sec. for G, holds up well until stimulation is applied in the extreme periphery. The R and G processes, meanwhile, diminish in excitability. The broken line with the filled circles among the foveal curves of Fig. 13 represents data taken with a time delay of 1.5 sec. Here the Y process curve shows two humps, one each at R and G. This was interpreted by Motokawa to mean that the physiological

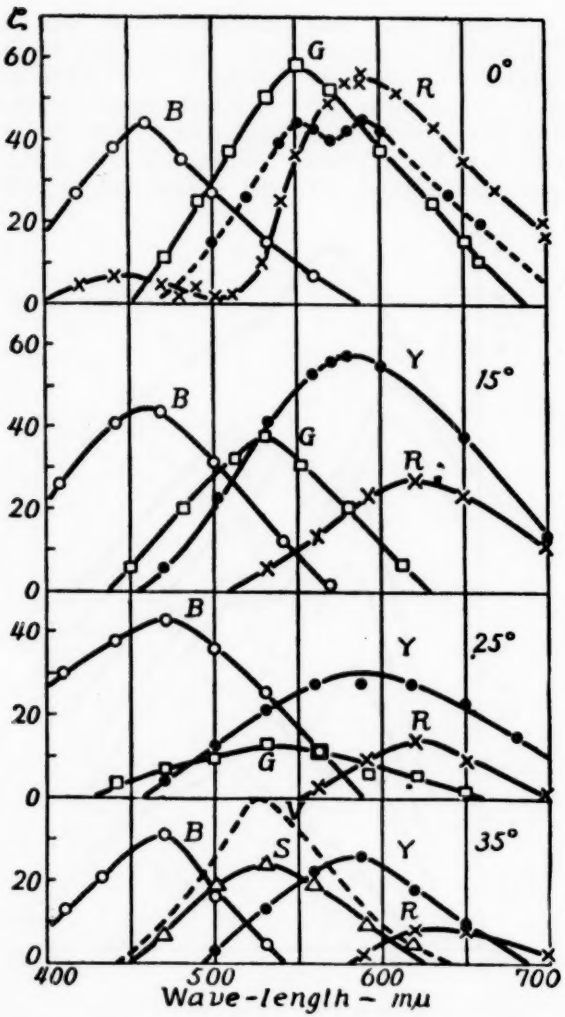

FIG. 13. EFFECT OF RETINAL POSITION ON THE PHYSIOLOGICAL SENSATION CURVES FOR THE NORMAL EYE. [MOTOKAWA (36).]

method reveals no independent Y process in the fovea. Outside of the fovea, however, the Y process at 15°, 25°, and 35° shows up to be stronger than the R and G. In the fovea, the R and G have maxima somewhat different from what appears parafoveally, and the R has a slight rise in the short wave lengths at about 440 mμ. Another point to note is that at 35° the middle of the three processes is elevated over what it was at 25°. This, Motokawa marked S (scotopic) to distinguish it from the G process. The broken curve, V, in the bottom section of Fig. 13 is a scotopic curve determined with a flicker photometer. Since both S and V are

seen to cover the same spectral range with maxima at the same position, they are interpreted as a reflection of rod activity.

The deuteranomalous data in Fig. 14 need little comment. The Y process is shown as absent from the fovea, but appears in the periphery. ζ-time curves showed the protanope to have a depressed excitability at about 1 sec. and the deuteranope at about 2 sec. In the physiological sensation curves, the protanope lacked the R process curve and the deuteranope the G curve. Data on the physiological sensation curves

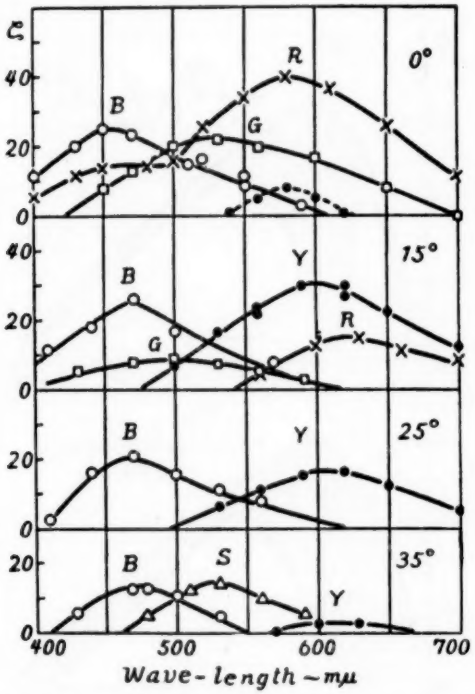

FIG. 14. EFFECT OF RETINAL POSITION ON THE PHYSIOLOGICAL SENSATION CURVES FOR THE DEUTERANOMALOUS EYE. [MOTOKAWA (36).]

The weak G process diminishes rapidly and drops out at 25°. The S curve again turns up at the extreme periphery.

Dichromats

Data on dichromats were presented in several papers (9, 36, 65). In general, alterations in the ζ-time and physiological sensation curves were as would now be expected. The for dichromats were very clearly in agreement with prediction. In the ζ-time data as presented in papers by Motokawa (36) and Motokawa and Suzuki (65), there appears to be contradiction in the way the protanope behaves. More helpful are the data given by Ebe, Isobe, and Motokawa (9), where it is made quite evident that excitability in the protanope is shifted to the longer times

corresponding to the shorter wave lengths.

Motokawa and Suzuki (65) reported that if, in the case of the deuteranomalous *S*, the intensity or size of the pre-illuminating stimulus was reduced, the weak elevation in the ζ-time curves disappeared and the result could not be distinguished from that of a deuteranope. In this paper they also presented further data on the ζ-λ relationship for dichromats and anomalous trichromats.

It is worth mentioning, in passing, that the electrostimulus method is claimed to be more sensitive than conventional tests in detecting the presence of color deficiency (36).

Inhibitory Action of Colored Light

Motokawa opened up a new area for his electrovisual investigation by the accidental discovery of what he calls selective inhibition (47). During some experiments on summation (46) he happened to be working in a room that was not completely dark but was faintly illuminated by green light. The effect of this changed condition was to deform the ζ-time curve in this way: In the normal eye with white light the presence of the green illumination caused the curve to exhibit two maxima instead of one and thus to resemble data obtained from a deuteranope. Two hypotheses to explain the disap-

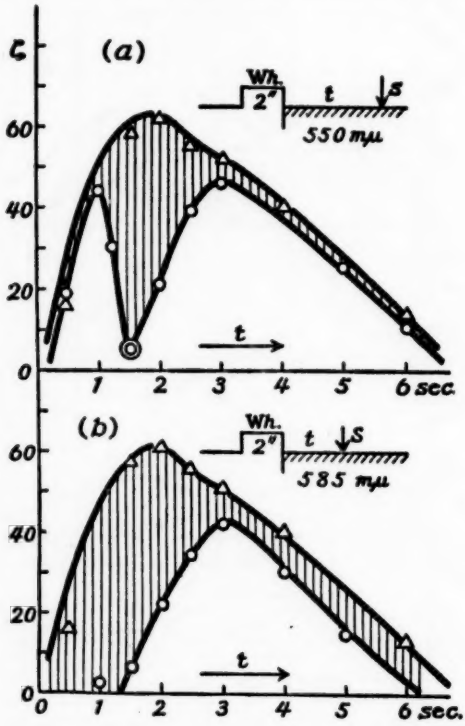

FIG. 15. ALTERATION OF THE ζ-TIME CURVE OF WHITE LIGHT BY INHIBITORY COLORED LIGHTS. THE UPPER CURVES ARE THE NORMAL ζ-TIME RELATIONS FOR THE WHITE LIGHT. THE LOWER CURVES ARE DUE TO LIGHT OF 550 AND 585 mμ. THE SHADED AREA IS THE AMOUNT OF INHIBITION. [MOTOKAWA (47).]

pearance of part of the curve were entertained. First, that the effect was simply selective adaptation. This was rejected when it was found that the green light had to be present during the time course over which excitability was measured. It could not be removed with the termination of pre-illumination. The second hypothesis was that the green light had a selective inhibitory influence on the development of the G process.

An example of these kinds of data obtained at the fovea of the normal eye is shown in Fig. 15 where in the upper curve light of 550 mμ is seen to reduce greatly the G process and leave the R and B processes intact. The shaded area represents the inhibitory effect. In the lower curve light of 585 mμ eliminates the R and G processes and leaves the B process. Motokawa's theory was that if it is assumed that white light arouses the R, G, and B components in some definite proportions and that the inhibitory light withdraws some of the excitation process from particular components, the excitability curve would be the result of what is left. This process apparently operates on a complementary basis.

Figure 16 shows how an analysis by inhibitory lights applies to yellow

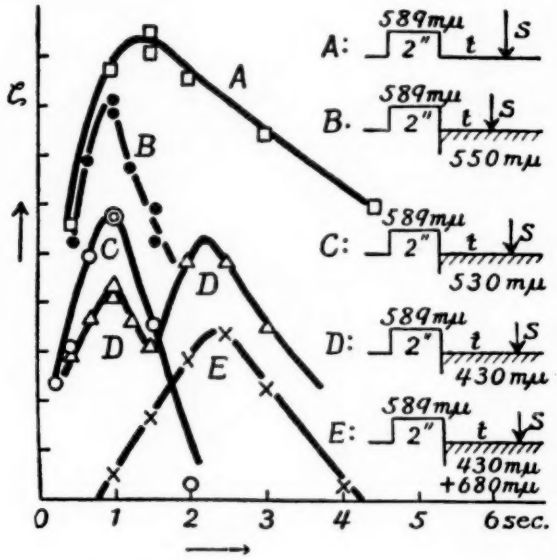

FIG. 16. ANALYSIS OF LIGHT OF 589 mμ BY SELECTIVE INHIBITION. [MOTOKAWA (47).]

pre-illumination of 589 mμ in which the maximum for ζ in the normal fovea occurs at about 1.5 sec. as shown in curve A. The effect of light of 550 and 530 mμ is to inhibit the G component of yellow light, but not the R. Light at 430 mμ takes out some of the R and B, and leaves the G intact. A mixture of 430 mμ and 680 mμ eliminates the R completely and leaves the G. Motokawa felt that this again demonstrated the dual nature of the effect of yellow light in the fovea.

For the parafovea, where he reported having found a separate Y process (36) in both normal and deu-

teranomalous eyes, he concluded on the basis of a similar analysis using selective inhibition that the Y process is unitary and not composed of R and G. However, it is not clear from the data presented in this paper that this is the case.

The next report on this matter was by Motokawa and Suzuki (67) and dealt with the problem of where, in time, to introduce the inhibitory light for greatest effectiveness. The first thing the authors did was to measure inhibitory effectiveness, as related to the R, G, and B processes, by presenting a flash of inhibitory light at varying intervals before and after the electric stimulus was given. The measure ζ was again used and inhibition was expressed as the difference in ζ-units between a normally determined excitation value and that obtained when the inhibitory light was used. An example will help here. If Motokawa wished to measure the inhibitory effect of light of any wave length on the R process, the ζ-value taken 1 sec. after stimulation with white light would first be found. This would be maximum for a normal eye. Then an identical measurement of ζ would be obtained with pre-illumination ceasing at a moment .5 sec., say, prior to the electric stimulus, which was always delivered at precisely the 1-sec. mark. The difference between these two values of ζ would be the inhibitory effectiveness. To study the inhibitory effect on G and B

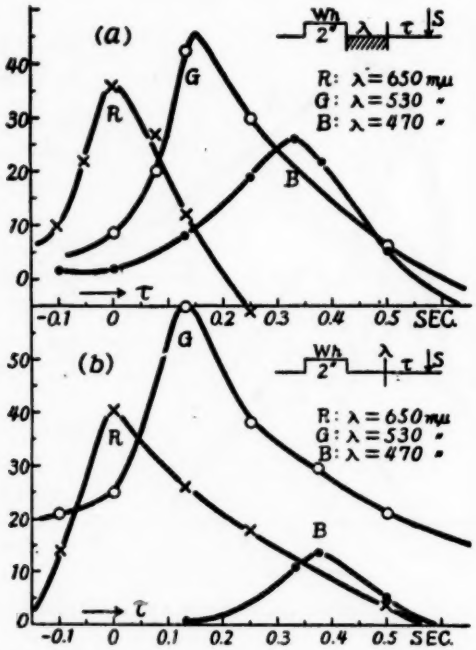

FIG. 17. INHIBITORY EFFECTIVENESS OF COLORED LIGHT ON THE R, G, AND B PROCESSES OF THE NORMAL EYE. SEE TEXT. ABSCISSA: THE INTERVAL BETWEEN THE INHIBITORY LIGHT PULSE AND THE ELECTRIC STIMULUS. [MOTOKAWA AND SUZUKI (67).]

processes, the same thing would be done, except that 2 and 3 sec. would have to be used. The interval between the inhibitory flash and the electric stimulus was called the inhibition-electric stimulus (I-S) interval. It was considered positive when the flash preceded the current and negative when it followed. Figure 17 shows the effectiveness of light of 650, 530, and 470 mμ on the R, G, and B processes when the fovea of a normal eye is illuminated. The upper curves are the result of presenting the inhibitory light immediately after pre-illumination and turning it off at fixed times before the electric pulse. The lower curves are the effect of a 1/20-sec. flash. Both sets agree to the extent that the optimum I-S intervals turn out to be zero, .13, and .37 sec. for R, G, and B, respectively.

Stimulation of the periphery, 15° from the fovea, revealed a fourth I-S interval as measured from a base 1.5 sec. after pre-illumination. This is the locus of the ζ-time maximum of the Y process. This I-S interval was at .07 sec. and was noted to lie between the R and G intervals.

At the extreme periphery, 40° from the fovea, another optimum I-S interval appeared at .05 sec. Since this new interval could not be demonstrated to be affected by the wave length of the inhibitory light, it was suggested as being due to scotopic or rod action.

Table 1 summarizes the characteristics of the basic retinal processes in the normal eye as shown by Motokawa's method using a 2° stimulus spot. The authors felt that the time relations in photopic vision stood together quite well but that the

TABLE 1

CHARACTERISTICS OF THE BASIC RETINAL PROCESSES OF NORMAL MAN AS REVEALED BY ELECTRICAL STIMULATION OF THE EYE
[FROM MOTOKAWA AND SUZUKI (67)]

Kind of Retinal Process	*True Crest Time in Seconds*	*Optimum I-S Interval in in Seconds*	*Maximum of Spectral Locus*
Photopic Processes			
R	1.0	0.00	590 mμ in fovea 620 mμ in periphery
Y	1.5	0.07	585 mμ
G	2 (2–2.25)	0.15	550 mμ in fovea 530 mμ in periphery
B	3 (2.75–3)	0.35	460 mμ
Scotopic Process			
S	2.0*	0.05	520 mμ

* This excitation time is properly 4.5 sec., according to more recent measures (**49, 51**). See below.

scotopic process was a thing apart. Classical attempts to determine whether photopic or scotopic reactions are the faster were claimed to be irrelevant. The speeds of these reactions were shown to overlap, depending on what aspect of the photopic process was measured. Nevertheless, Motokawa and his associates proceeded to examine the nature of the scotopic process further in several recent papers.

Motokawa and Ebe (**49**) and Motokawa, Ebe, Arakawa, and Oikawa (**51**) reported, first, that while the ζ-time curves at the fovea depended little upon the duration of pre-illumination, those at the periphery had their maxima shortened by increasing durations. But the maxima remained the same if measurements were dated back to the *onset* of pre-illumination. This finding was correlated with "on"-type elements associated with the rods. Second, the true maximum time for the photopic process was amended to 4.5 sec., or the slowest in the retina (cf. Table 1). Third, when ζ was measured at 4.5 sec. for flashes of spectral light delivered to a retinal area 20° from the fovea, the resulting ζ-λ curve coincided almost exactly with the scotopic visibility curve.

In another paper Motokawa and Suzuki (**66**) continued the attack on the quantitative aspects of inhibitory lights. What they considered was the way intensity and wave length of inhibitory stimuli affected the amount of inhibitory effect. The experiments were done with the optimum I-S intervals for the R, Y, G, and B processes as described above. The general results indicated, first, that inhibition increased with an increase in the intensity of the inhibitory light. This was true for all of the color processes. Second, inhibition was related to wave length more or less the way ζ is related to wave length. Therefore, the inhibition distribution curves closely resembled the physiological sensation curves shown in Fig. 13. Indeed, the maxima of these curves were claimed to correspond within the limits of experimental error.

Microstimulation Studies

Two studies have been carried out by Motokawa, *et al.* (**50, 52**) using the microstimulation technique of Hartridge (**19**) to obtain small areas of pre-illumination. The work on electrical excitability reported up to this point had been done with test patches of 2°. Now a pre-illumination area of 2′ of arc was employed in, or near, the center of the fovea and at 20° from it. It was found that in the fovea the R, Y, G, and B processes existed as revealed by the ζ-time and ζ-λ curves. At the very center, however, no measurable Y response and only a weak B were observed. Near the center, or out to about 1°, the R and G process responses predominated, and beyond 2° the Y and B processes took over. The maxima of the three physiological sensation curves obtained with the 2° patch had been 585, 550, and 460 mμ for R, G, and B, respectively. For the 2′ patch these become four in number at about 610, 575, 520, and 460 mμ for R, Y, G, and B, respectively. The three-components maxima were observed to correspond well both with Granit's maxima of 600, 530, and 460 mμ for the spectral sensitivity of modulators in the cat's eye, and with the general maxima in the sensation curves found by numerous investigators for color matching.

Work at 20° from the fovea confirmed the strong Y and B process

excitability and the weak R and G. The conclusion reached from these studies was that they confirmed and extended Hartridge's observations on the nature of the foveal receptor mechanism.

Evidence from the Retinae of Frogs

Since this review is intended to treat the results of electrical analysis of the human eye, no attempt is made here to do other than mention the work that Motokawa and his associates have done on frogs and toads. It should be noted, however, that seven papers have been submitted to show that much the same relationships found in human eyes can also be demonstrated in frogs (**35**, **55**, **56**, **57**, **61**, **63**, **79**). The method was essentially the same as has been described, except that the indicator of excitation now is the action potential taken from the optic nerve. Barring the difference that the action of the amphibian eye was sluggish, the ζ-time curves showed about the same characteristics as the human curves. The white, R, G, and B elevations of the ζ-time curves could be clearly seen in the data, and the dependence of ζ on intensity was evident. The phenomena of selective inhibition were demonstrated in a manner analogous to that found in the human eye. Indeed, even color contrast, to be described for human subjects in the next section, was observable in the responses of the frog's retina.

The existence of these and other phenomena in the nonhuman eye was emphasized by Motokawa as indicating anew the basic physiological nature of his electrostimulation method. It is of especial interest that work by Motokawa, Iwama, and Tukahara (**63**) and Tukahara (**79**) on modulators and dominators gave results that correspond well with Granit's studies.

III. *Summation, Contrast and Optical Illusions*

The generality of using an electrically aroused phosphene in investigating the effect of photic excitation processes was shown next to extend to such phenomena as summation, simultaneous and successive induction, and numerous optical illusions.

TABLE 2

SUMMATION EFFECTS IN VALUES OF ζ FOR DIFFERENT DEGREES OF SEPARATION BETWEEN STIMULI
[MOTOKAWA (**41**)]

Angular Separation in Degrees	*Single Spot*	*Five Spots*	*Summation Effect*
0	54.5	77.0	22.5
1	54.5	75.5	21.0
2	54.5	68.0	13.5
3	54.5	64.0	9.5
4	54.5	58.5	4.0
6	54.5	54.5	0.0

Spatial Summation

The first paper on summation was by Motokawa (**41**), who studied the effect of using, singly and together, arrangements of variously separated test patches. Pre-illumination with a stimulus pattern of four spots regularly arranged around a center spot was compared with a single spot for the effect on ζ. Each spot was a circle of 1° in diameter, and the angular separation between the four outside circles and the center patch was varied. The intensity of the test patterns was 1000 lux and the exposure was .13 sec. Table 2 shows the summation effect due to the use of

five spots over one spot. Summation is seen to disappear when the separation in the multiple-spot figure is about 5°. That the amount of summation depends upon intensity was shown by separate measurements using 5200, 280, and 180 lux. These intensities gave limiting values of separation of 13.6°, 7°, and 3.5°, respectively. It was pointed out that Granit (**14**) found summation in a four-spot experiment, using flicker, with a separation of 2° and an intensity of 100 lux.

The effect of retinal position was measured with one or both halves of a circular patch. When both halves were used, they were separated by 2°. The results appear in Table 3. The

TABLE 3

SUMMATION IN VALUES OF ζ AT DIFFERENT POSITIONS ON THE RETINA
[MOTOKAWA (**41**)]

Degrees from Center	*Single Patch*	*Double Patch*	*Summation Effect*
0	63.5	69.0	5.5
20	50.0	55.5	5.5
40	42.0	48.0	6.0

intensity was 1000 lux. Here the ζ-values are seen to decrease, but the summation remains the same, indicating a greater percentage increase at the periphery than at the center of the visual field. When the intensity was reduced to 8 lux, however, the summation effect could no longer be seen beyond 10° distant from the fovea.

ζ and the Size of the Area Stimulated

This relationship was studied at 1000 lux for the fovea and at 20° and 40° in the periphery. The exposure time was .13 sec. The results for a variation in spot diameter of one log unit are curves of the same general shape as the ζ-log I curves shown in Fig. 6.

Summation of Colored Light

Motokawa found scant evidence of this kind of summation in an experiment in which he had a split-field, circular patch of 2° diameter that could be illuminated with either one or two colors. Filters were used to obtain red (620–740 mμ), green (490–550 mμ), and blue (410–490 mμ) illumination. Stimulation was confined to the rod-free area. The effect of homogeneous illumination of the whole circle was to increase markedly the ζ-value. This doubling of the area caused ζ to rise from 42 to 75 for red, from 43 to 71 for green, and from 45 to 69 for blue. When the patch was illuminated with pairs of lights of different hues, it was found that practically no interaction existed when the colors were spectrally separated as in the red and blue. Some occurred to raise the ζ-value when the pairs lay closer together. This is explained by Motokawa in terms of the three-component theory on the basis that individual lights will stimulate more than one color process when they are near each other in wave length, but not when they are widely separated.

In another experiment Motokawa (**46**) looked into the matter of summation by direct color mixing where the stimulus arrangement allowed no separation between the areas where light fell on the retina. He found that a mixture of yellow (580 mμ) and blue (450 mμ) produced no significantly larger values of ζ than was true when the two lights were taken separately. This was also true when wave lengths of 650 and 470 mμ were mixed. He concluded that the law of additivity normally found for color mixing did not hold for the physiological meas-

ure of excitation as determined by his method.

The next general problem that Motokawa treated was that of brightness and color contrast (37). The method again was a simple extension of his general procedure. What he did in this case was to measure ζ by introducing the electric test stimulus after a contrast situation had been set up. The insets in Fig. 18 and 19 show how the stimuli were ordered for brightness contrast.

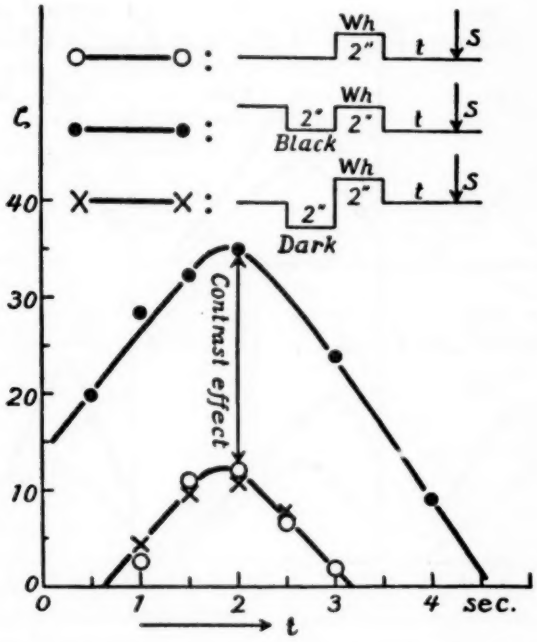

FIG. 18. EFFECT OF SUCCESSIVE BRIGHTNESS CONTRAST ON THE ζ-TIME CURVES. [MOTOKAWA (37).]

Brightness Contrast

To study the effect of successive brightness contrast on electrical excitability, a 2° test spot was seen against the background of a screen held at .05 lux. The test spot was either black (no light) or white (84 lux). These were presented successively, as indicated in Fig. 18, where the results are also shown. ζ is raised, when the black stimulus precedes the white (upper curve), over what it is when white alone is used (lower curve —open circles). To rule out the effect of adaptation, the screen was darkened for 2 sec. before the white patch was given. This, as shown in the crosses of the lower curve, had no effect. The amount of contrast was further shown to depend, somewhat, on the duration of the black stimulus, and more particularly, on the intensity of the adapting field. The contrast effect increased as the intensity of the adapting screen was raised, since raising the field level increased the "blackness" of the dark spot.

Simultaneous contrast was also investigated by placing a bright patch

within a dark, concentric ring. The effect of this was to raise ζ from 13 to 34.5. The difference in excitability in this instance was considered due to contrast.

Color Contrast

Successive color contrast is shown in Fig. 19, where it is seen that if white light is preceded by colored light, the ζ maxima occur at times corresponding to the complement of the colored light used. White light, however, has no effect on the excitability curve of white light. This is seen in Fig. 19 (*a*). Further experiments showed the contrast effect to be dependent on the intensity of the inducing light, but not on that of the white test light.

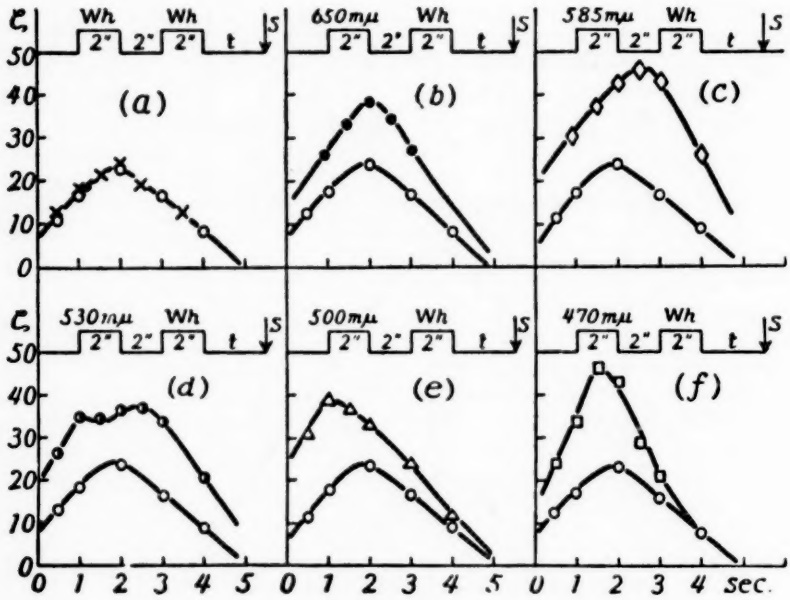

FIG. 19. EFFECT OF SUCCESSIVE COLOR CONTRAST. [MOTOKAWA (**37**).]

Motokawa's supposition, as set forth in this paper (**37**), was that an optical image formed on the retina produced in the surrounding area a field of induction that could be mapped out by his electrostimulus method. Now he proceeded to demonstrate this in two papers on retinal induction (**45**, **48**).

Optical Illusions

In the first report Motokawa measured the induced field surrounding geometrical figures (squares, triangles, and circles) and certain specialized configurations such as the Hering squares, the Wertheimer-Benary contrast figures, the Müller-Lyer illusion, and the Landolt ring (**45**). The procedure was to present an intense, yellow (sodium lamp) stimulus to the retina in the shape of the inducing figure. The exposure of this was for 2 sec. Then, after a delay of 3 sec., a small white test light was presented in different parts of the retina surrounding the inducing yellow exposure. This second exposure

was also for 2 sec. Finally, a 100-msec. electric pulse was applied to the eye after a second delay of 1.5 sec. (the ζ maximum for the Y process) for the purpose of finding the threshold phosphene. Contrast was defined as before; i.e., by the difference between the ζ-value determined for white light and the ζ-value determined after the above procedure.

The startling finding was that a gradient of field, as defined by this electrical measure of a contrast effect, existed around a given stimulus figure. This excitability field was strongest at the margins of the figure and fell off to zero with distance from the boundary. The rate at which the field strength declined depended upon where, with respect to the boundaries of the figure, the field was measured. Measures of field strength were used by Motokawa for constructing geometric representations of the gradients surrounding different figures. For example, the shape of the field surrounding a circle was circular, but the field surrounding a square was in the form of a cross, with the field falling off most sharply at the corners.

The same general procedure was then used to explore the retinal surrounds of the illusions mentioned above and it was shown that the fields of retinal induction were distinctively patterned by the figures involved. The suggestion was made by Motokawa that the experience of illusion may perhaps be accounted for by deformations in the fields of retinal induction surrounding a figure. These fields may be due either to the figure itself or to some other configurations in the vicinity. In either case, physiological processes serving the retinal image are warped or modified in a manner characteristic of the illusion.

Propagation of Induction Effects

This very fascinating work was continued in the second paper (**48**) on color contrast effects. Motokawa at this time introduced some new terminology. By *direct induction* he means the residual effect left in the *same* area of the retina by stimulation with colored light. It may be measured by his electrical method in the manner already described; i.e., by comparing ζ for white light alone with ζ for white light that has been preceded by colored light. So if yellow light is used, excitability will increase in the region of the ζ-time curve where the B process is, or, in this case, at 3 sec. Now if the yellow light is followed by the proper amount of exposure to blue light before the test with white light is made, the enhancement at 3 sec. disappears and the ζ-time curve is identical to the simple white light curve. This is called *neutralization*. If the intensity of the neutralizing blue light is too high, overneutralization occurs.

Motokawa then studied the effect of the intensity of the neutralizing light in direct induction, and the temporal position of the neutralizing light with respect to the inducing light. With regard to the latter, he found that the contrast effect of an inducing light persisted for 15 sec. or more and that the effectiveness of a neutralizing light increased the later it followed the inducing light.

By *indirect induction* is meant the residual process that exists in an area adjacent to the area stimulated. If the ζ-time curve is obtained using a white test-patch in an area next to where yellow light has acted, the maximum will be at 1.5 sec., not at 3 sec., as in the direct induction case where the same retinal area is involved. This effect may be neu-

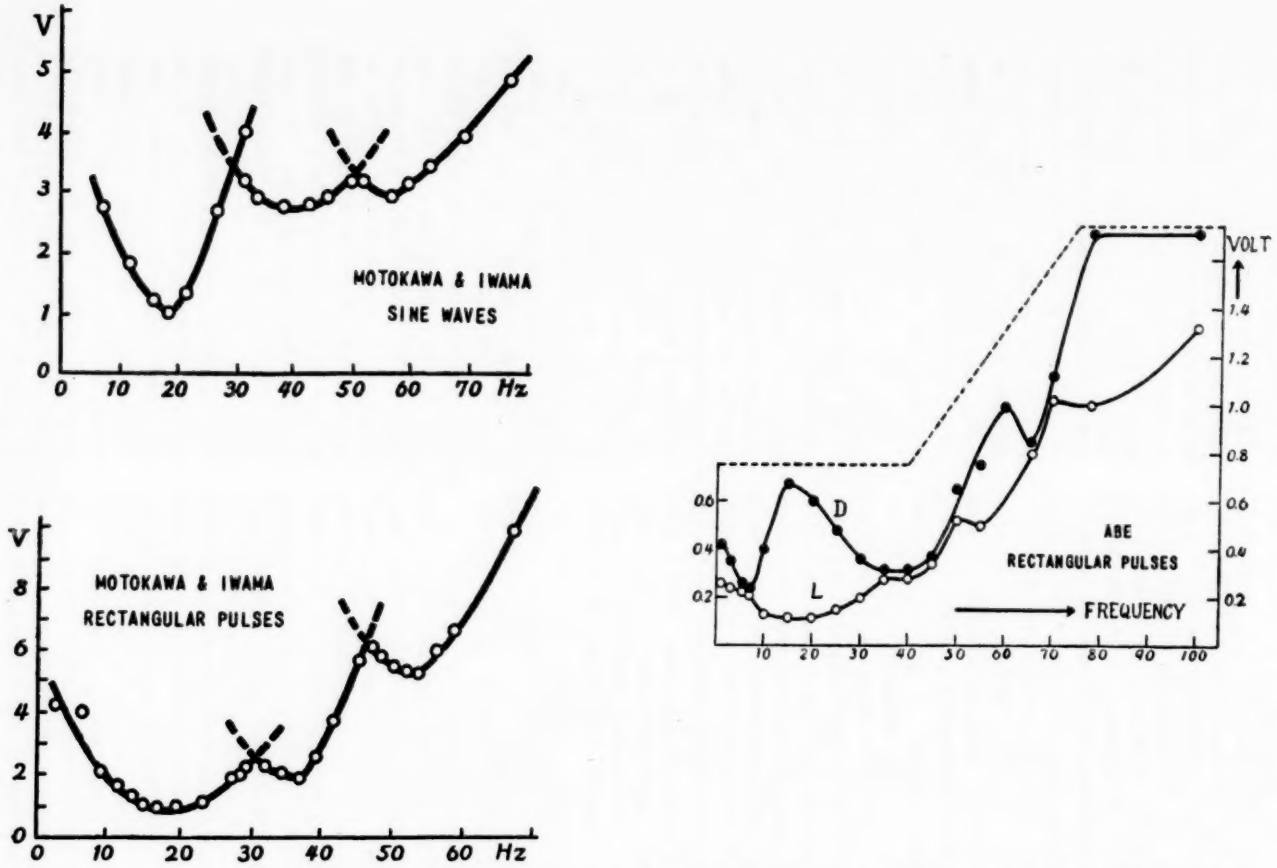

FIG. 20. STRENGTH-FREQUENCY RELATIONSHIP FOUND FOR THE LIGHT-ADAPTED EYE BY MOTOKAWA AND IWAMA (60) AND ABE (1). CURVES L AND D DENOTE LIGHT- AND DARK-ADAPTED EYES, RESPECTIVELY.

tralized by a light of the same color as the inducing one.

The previous work (**45**) had shown that retinal field gradients were rather extensive. Indeed, they were astonishingly large, sometimes reaching out from the area stimulated as far as 4° to 6°. By setting up an "inducer-detector" arrangement in terms of stimulus lights, Motokawa next showed that the effects of indirect induction were propagated over distances of 46° of arc in the retina. This propagation effect would not pass through the blind spot nor across a barrier formed by the image of a white light stimulus.

The whole area of vision that Motokawa has touched upon in the studies reported in this section has been one in which little real progress has been made, in spite of great interest, especially on the part of psychologists. The discovery that measurable alterations in the electrophysiology of the retina can be demonstrated to occur during contrast and illusion processes is most welcome. The verification of this should afford an engaging challenge to many workers in the field of vision.

IV. *Stimulus Strength-Frequency Relationship*

It has long been known from work on tissues other than the retina that optimum frequencies of excitation exist at which thresholds to intermittent electric stimulation are lowest (**27**). U-shaped curves result when threshold current (I) is plotted against log frequency (*n*) (**7**). The I-log *n* curves are symmetrical about the optimum frequency and their theoretical importance has been discussed in detail by Hill (**21**), Coppée (**7**), Katz (**24**), Schaefer (**70**), and others. Rohracher (**69**) showed in 1935 that the stimulus strength-frequency curve for just noticeable flicker phosphenes passed through a minimum at about 20 cy./sec. These data were obtained with sine waves for the light-adapted eye. Schwarz (**71**, **72**, **73**, **74**, **75**) verified and extended Rohracher's observation that only one frequency of maximum stimulus effectiveness existed for the light-adapted condition. Motokawa and Iwama (**60**) and Abe (**1**), however, have reported three and five minima, respectively, for electrical thresholds during light adaptation, although in another place Motokawa reported data on two minima, while mentioning three and four (**38**). Abe, further, stated that the minima obtained during adaptation to darkness were not the same as those found during the light condition. Some of these results are shown in Fig. 20. It is interesting to note that, for Abe, the 20 cy./sec. minimum disappears altogether during dark adaptation. Motokawa considered this essentially a resonance phenomenon with the most conspicuous natural frequency of the eye to be at about 18 cy./sec. for the light-adapted condition. He offered, in support of this opinion, evidence that the period of oscillation into which the eye was thrown by a sensitizing electric shock was 54.85 msec. (**44**). This works out to be about 18.2 cy./sec. and corresponds well to the strength-frequency data.

This is the first, and to date the only, opportunity available to compare any of the findings of the Tohoku laboratory with those of other investigators who have recently looked into visual electrostimulation problems. It has already been mentioned that Rohracher and Schwarz found only one minimum in the strength-frequency curve. Gebhard (**11**, **12**), using several different wave forms, also found but one unequivocal minimum. This was at about 20 cy./sec. as shown in Fig. 21.

The strength-frequency relationship for the dark-adapted eye was studied in considerable detail by Schwarz (**75**). Abe's findings for this condition are completely at variance with Schwarz' data. Nevertheless, nothing can be concluded from all this. Either there is only one minimum in the stimulus strength-frequency curve or there are more. There are objections to be raised to all of the investigations and it is evi-

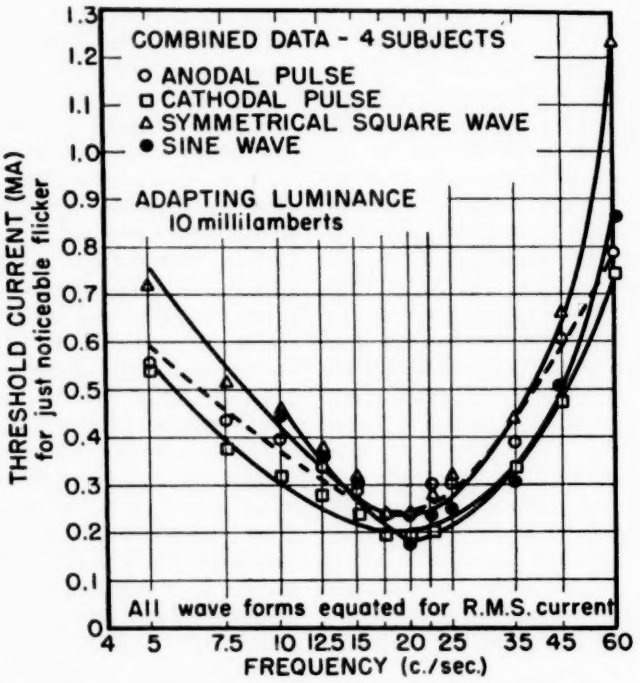

FIG. 21. STRENGTH-FREQUENCY RELATIONSHIP FOR THE LIGHT-ADAPTED EYE. [FOUND BY GEBHARD (**12**).]

Abe's supposition that the absence of the 20 cy./sec. minimum under dark adaptation was due to the low electrical excitability of the rods was pursued further by Tukahara and Abe (**80**). These authors obtained ζ-time curves on eyes pre-illuminated with intermittent white and colored lights of different intensities and at different positions on the retina. They concluded from their data that the natural resonant frequency of the rods, when light was acting on the eye, was 20 cy./sec. Unfortunately, dent that this problem requires restudy.

V. *Measurement of General Fatigue*

People have been trying for years to tease an indicator of fatigue out of photic flicker effects (**6**, **29**, **76**). The success of this effort has been small. Notwithstanding the poor record of using light, Motokawa and Suzuki (**64**) introduced in 1948 a method of measuring fatigue that made use of the electrically aroused

phosphenes. The procedure was to impress 20 cy./sec. pulses across the head in the manner by now familiar. Under a normal, rested condition, the thresholds S_1 and S_2 were obtained for the appearance and disappearance of flicker, respectively, by increasing and decreasing the stimulus voltage at a uniform rate. S_1 was found always to be higher, so the difference ΔS_o was gotten by subtracting S_2 from S_1. Therefore,

$$\Delta S_0 = S_1 - S_2.$$

This process was then repeated after fatigue to obtain the difference, ΔS. The comparison between ΔS_o and ΔS was made by finding their difference. $\Delta S - \Delta S_o$, therefore, is the measure of fatigue that is reported to be sensitive enough to be useful.

Correlation with Oxygen Deficiency

In a paper that appears to have been written earlier than the above, Motokawa and Iwama (**58**) reported work on electrical excitability and oxygen deficiency. Here also were used the appearance and disappearance thresholds for flicker and the difference between them. Measurements were taken under moderate light adaptation in a low pressure chamber over a range of pressures equivalent to over 20,000 feet of altitude. The main result was that while the means of the appearance and disappearance thresholds stayed about the same, the former rose somewhat while under low oxygen pressure, whereas the latter fell. The difference between them, then, turned out to be an indication of oxygen deficiency and can be seen to be like the ΔS term referred to above. The authors also measured the sensitivity of the eye to light during decreased oxygen pressure and concluded that their new-found measure of electrical excitability was the more suitable.

Effect of Work

Suzuki (**78**) next carried the method over into the field of exercise and compared oxygen consumption with thresholds of electrical flicker. The electrical data before and after work were expressed in terms of $\Delta S - \Delta S_o$. The oxygen consumption was measured by conventional methods and was expressed as the difference

$$O - O_o$$

where O_o is the quantity of oxygen consumed per minute at rest and O is that consumed during work or in the course of recovery from work. In a series of experiments on the bicycle ergometer, running and stair climbing, the data from the two methods ran parallel to each other with remarkably small discrepancies. Suzuki concluded that the two methods were measuring the same thing and that $\Delta S - \Delta S_o$ was perhaps the more sensitive measure.

A point of difference that should be mentioned is that whereas Motokawa and Iwama (**58**) found the mean values of appearance and disappearance to stay the same for reduced oxygen pressure, Suzuki's results showed that S_1 and S_2 tended to change in the same direction, while at the same time their differences were changing. The *difference*, nevertheless, continued to be the critical feature of the measure.

Suzuki (**77**) also studied the course of $\Delta S - \Delta S_o$ during the daily activities of workers. Three classes of workers, chosen on the basis of the estimated amount of fatigue involved in performing their jobs, gave the results shown in Table 4. The data are not extensive, but there is little overlap. These findings offered

further evidence that Motokawa's method was measuring something useful and real about fatigue.

Research on the Method

It had been mentioned by Motokawa and Iwama (**58**) that the rate at which the appearance and disappearance thresholds were approached seemed to be important in establishing the value of the thresholds. The matter of time was not well controlled in their experiment. The rate was controlled manually and the experimenter stopped varying the current at a signal from the subject. The experimenter's reaction time, as well as the subject's, undoubtedly caused a certain amount of overshooting. Mita, Abe, and Byonshik (**32**) carefully controlled such factors in a study in which they investigated the dependence of ΔS on the intensity of ambient illumination, the frequency of the flicker employed, and the rate of altering the stimulus current. They used a motor drive to control the rising and falling stimuli and a device that, at the subject's signal, recorded where the stimulus value was at threshold. This eliminated the experimenter's errors, but, of course, not the subject's reaction time.

TABLE 4

MAXIMUM DEGREE OF FATIGUE IN INDUSTRIAL WORKERS AS MEASURED BY MOTOKAWA'S METHOD
[AFTER SUZUKI (**77**)]

Severity of Work	*Job Examples*	*Maximum Fatigue* (Range of $\Delta S - \Delta S_0$ in mV)
Light ($N=4$)	Doctor, nurse, clerk, designer	196–216
Medium ($N=11$)	Locomotive engineer, telephone girl, telegraph operator, etc.	212–380
Heavy ($N=5$)	Stoker, coolie	432–635

The results indicated that the measure ΔS could be analyzed into a factor, S, which is the mean threshold $(S_1+S_2/2)$ and the reaction time, T, for the minimum stimulus. They proposed then, the equation

$$\Delta S = KS - 2TV,$$

where K is a constant and V is the rate of altering the stimulus current.

There are several psychophysical aspects of the data presented that require more study. For example, the frequency of the current pulses must be kept constant, since thresholds measured at values above and below 20 cy./sec. were observed to give somewhat larger measures of ΔS. This would appear to mean that the presence or absence of flicker can be more sharply discriminated at 20 cy./sec. than at any other frequency. Is there a significance in this for the strength-frequency relation? Another question is: why should threshold be proportional to the rate at which the current is raised or lowered only when adaptation is held constant? These are but a few of the interesting problems raised by this analysis. The only safe conclusion for the use of Motokawa's test of fatigue is that ΔS should be measured under carefully controlled conditions.

CONCLUSION

There can be little doubt that the reported work of Motokawa and his associates is exciting stuff. It represents a fresh and unique approach to vision that appears to promise great things. Unfortunately, this cannot be said without reservation.

The work of the Tohoku laboratory has come upon the rest of the scientific world so recently, and in such volume, that there has been little opportunity for visual workers to digest it, to say nothing of confirming it. An evaluation of Motokawa's findings cannot be made in any serious sense until a considerable amount of his experimentation has been redone, verified, or modified, as the case may be. For the present, one can only say that the work looks good. Indeed, it almost looks too good, in that the data of electroanalysis and that of more conventional experimentation fit together, in many respects, so beautifully.

It is possible, of course, to point out some obvious shortcomings. These were touched upon briefly in the introduction. The first has to do with the method of measuring the electrical thresholds. The reading of voltage is not the method of choice in this kind of work, although if proper precautions are taken, voltage measurements may be reasonably satisfactory. For example, a frequent redetermination of E_o is certainly needed to eliminate the consequence of voltage changes due to variations in resistance. Motokawa probably does this, although he does not make it clear that he does.

The second difficulty concerns the amount of data Motokawa has used in presenting his material. Complete tables of the measures obtained are never given. Consequently, no independent estimate of such matters as the variability of measures and the significance of differences is possible. No statements are made about how many measures were used to determine any given point on the numerous curves published. It can only be observed that the points plotted from the data generally can be fitted very well by smooth curves. Data are always presented as typical examples of the measures obtained. The extent to which atypical data were collected cannot be judged. Finally, the number of subjects measured is rarely given. Since a more complete reporting of the data would do much to satisfy critics in many laboratories, this matter was referred to Professor Motokawa. In two personal communications[7] to the writer he makes the following observations. First, he stresses the importance of thoroughly trained observers. In most of his experiment he used two or three subjects, and two of the most frequently used have had seven years of experience. He notes that other workers in Japan have failed to reproduce his results until after adequate, long-term training had been given. In this connection, he attaches great weight to the ability of *S* to reproduce his thresholds. Since each threshold determination takes a rather long time, numerous measures cannot be obtained. A search for quick methods of determining the electrical thresholds yielded only unsatisfactory results. Finally, since he believes that there are many kinds of receptors in the retina, each with its own electrical threshold, great care must be taken to be sure the threshold obtained is the one associated with the receptor process under scrutiny. Hasty threshold determinations, some tapping one process and some another, yield values that belong to different populations and, hence, cannot be averaged in any meaningful way.

The total work, nevertheless, remains impressive, and these criticisms are in no way intended to detract from it. This review cannot be a substitute for reading the papers. It is hoped that it will merely serve

[7] February 5, 1952 and June 1, 1952.

two purposes: first, to arouse some interest in the work, and second, to urge those workers who have available facilities to verify the published results. Such study should not be difficult, since the technique is clearly reported and appears not to be particularly involved. If Motokawa's work stands, it will unquestionably be one of the most important contributions to the physiology of vision in many years.

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SUPPLEMENT

Four new papers from Motokawa's laboratory have appeared since this review was written. Two of these report experiments on human subjects and two concern the application of the electrostimulation method to the visual processes of cats

Color diagrams. The first paper on humans by Arakawa and Oikawa (1) describes the construction of a color diagram from the three physiological sensation curves. It will be remembered that these curves are found by measuring ζ for each wave length when the time after pre-illumination is held at 1, 2, and 3 sec. The authors point out that the color diagram calculated from the ζ-λ data bears a close resemblance to diagrams constructed from color mixing results.

Strength-frequency problem. In the second paper Motokawa and Ebe report a new procedure that grew out of the work on the stimulus strength-frequency relationship (**5**). They found that the frequencies of alternating current that produced minima in the strength-frequency curves also produced the same effects as colored lights in the retinal induction situation. In retinal induction, pre-illumination by blue light, for example, followed by white light shifted the maximum of the ζ-time curve to the locus normally occupied by the yellow. Now it was shown that prestimulation by alternating current of 36 cps followed by white light produced the same shift to the yellow locus as the blue light. This relationship was worked out to indicate a functional correspondence between 33–37 cps and blue, 40–45 cps and green, 47–55 cps and yellow, and 60–100 cps and red. The conclusion reached was that the stimulus strength-frequency curve is a resonance curve revealing the composite nature of the retinal receptors.

A more limited conclusion about these matters was reached by Meyer-Schwickerath (**3, 4**) who found evidence that central and peripheral processes could be distinguished by electrostimulation. There is a minimum in each of Meyer-Schwickerath's curves. One is at 20 cps for the peripheral curve, and the other is at 33 cps for the central curve. Motokawa mentions this as supporting his and Ebe's findings. In the main, however, Meyer-Schwickerath's curves are unlike those found by other investigators. A recent paper by Bouman, ten Doesschate, and van der Velden (**2**), on the other hand, reports data in support of a single minimum in the strength-frequency curve.

Evidence from cats. The first paper on cats by Motokawa, Iwama, and Ebe (**6**) presents supporting evidence that properly chosen frequencies of alternating current play the same role in induction as colored lights. The correspondence between frequency and hue turns out to be about the same for men and cats.

The last paper is by the same authors (**7**). Using the method found successful with frogs and toads, they report that the ζ-time curves for cats passed through maxima at 1, 1.5, 2.25, and 3 sec. when the pre-illumination was red, yellow, green, and blue light, respectively. These times became longer when the body temperature of the animals fell during the experiments, suggesting that temperature may relate the mammalian response times to the more sluggish reactions of the amphibians.

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THE SZONDI TEST: A REVIEW AND CRITICAL EVALUATION

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The appearance of Deri's *Introduction to the Szondi Test* in 1949 (**15**) made available an organized body of theoretical and interpretive postulates for this technique. Szondi's *Experimentelle Triebsdiagnostik* appeared in 1947 (**40**) but an English translation has become available only recently (**41**). Thus Deri's text has been the primary reference for the test in the United States.

The Szondi Test, as any new proposition, in its exploratory phase has tended to produce isolated research with a minimum of communication among investigators. The *Szondi Newsletter* (**45**) established in 1949 cannot fulfill the coordinating function for which it was designed since most investigators seek other outlets to publicize their work. This state of affairs makes the present status of the technique somewhat ambiguous.

The problems of mediational rationale, validation, and personality theory inherent in the Szondi Test would seem to be in many ways representative of those generally encountered in multidimensional personality measures. A careful scrutiny of these problems should provide some general methodological principles for the investigation of projective techniques.

In the present evaluation of the Szondi Test, the rationale will first be examined in terms of the basic assumptions regarding the processes of test behavior and their psychological significance. These assumptions will then be evaluated in the light of the available research. Finally, there will be a general discussion of the theoretical and methodological problems characteristic of techniques such as the Szondi Test.

THE TEST

The administration and interpretation of the Szondi Test are presented extensively by Deri (**15**). A brief description will suffice for the present purpose.

Administration and Scoring

The Szondi Test materials (**42**) consist of 48 facial photographs of European psychiatric patients, with six representatives of each of eight diagnostic categories: passive male homosexuals, male sadistic murderers, epileptics in the intraparoxysmal phase, hysterics, catatonic schizophrenics, paranoid schizophrenics, manic-depressives in the depressive and in the manic phases. The test pictures are arranged into six sets, each containing one picture from each of the eight diagnostic groups.[1] The sets are administered successively to the subject with instructions to select the two "most liked" and the two "most disliked" pictures from each set. The subject thus has six opportunities to like, dislike, or ignore each class of pictures. The resulting pattern of 24 choices (12 likes and 12 dislikes) is represented by an eight-dimension profile. Each category is scored in terms of intensity (four to six choices is a "loaded" score and fewer choices yields an "unloaded" score) and direction of selection (a two to one imbalance of like and dis-

[1] Position of a given category within the sets is randomized.

like choices is a "plus" or "minus" score, four or more choices without such imbalance is an "ambivalent" score, and less than two choices in both directions is an "open" score).

Interpretive inferences from a single administration are considered tenuous and inadvisable. The standard procedure recommended is ten separate administrations with at least daily intervals.

Basic Assumptions

Szondi originally devised the test in connection with his theory of genetically determined personality (**43**). He postulated that the eight diagnostic syndromes of the test represent basic drives which are hereditarily determined. Both manifest physical appearance and personality are considered to be a function of genetic structure. The pictured patients have a definitely diagnosed psychopathology and, therefore, a known genetic structure and personality. The subject responds to the test pictures in terms of the relations of his own genetically determined basic drives to those of the pictured patients. The picture selections of the subject thus provide a basis for inference about characteristics of his personality. Szondi's constitutional orientation to personality probably accounts for the summary dismissal of the technique by many psychologists in this country. However, the merits of his genetic formulation need not be argued here since this rationale is not deemed essential to the technique.

Deri, a student of Szondi, seems to ignore his theory that personality characteristics are genetically determined, but continues to maintain that personality characteristics are reflected in the physiognomy. The diagnostic syndromes represented in the test are considered to be extreme manifestations of personality dimensions present within all persons. The subject's affective selections among the pictures are then due to interaction between eight need-systems within the individual and these needs as represented in the pictures. Thus, the homosexual pictures represent the "tender love" needs, the sadist pictures represent the "aggressive love" needs, etc. The basis of the subject's response seems to be essentially nonconscious in that he reacts in terms of stimulus qualities of which he is unaware.

As Schafer (**35**) has pointed out, Deri presents a body of interpretive hypotheses with a minimal elucidation of the mediational processes existent between stimulus and response. Deri makes the general assumption that the psychopathological dynamics of the various diagnostic syndromes are somehow communicated to the subjects through the medium of the test pictures. This general assumption embodies a number of important considerations.

The dynamics of diagnostic syndromes are assumed to be well established, with essential agreement as to clearly differentiating characteristics. These postulated dynamics are further assumed to be identical and equally well represented in all patients having a given diagnosis. Such faith in nosological clarity is not generally maintained in psychiatric practice and research.

That certain internal psychological states are reflected differentially in physiognomy has not been demonstrated. Deri seems to have created a paradox in adapting the test to a nongenetic formulation. In rejecting Szondi's genetic postulates she has dispensed with the assumption of constitutional origin, which is the essential link between personality and physiognomy. Although Deri's formulation of mental illness and personality has been more generally ac-

cepted than Szondi's, the role of physiognomy in her schema is more obscure.

The assumptions of nosological clarity with unique physiognomic manifestations are essential to Deri's hypotheses of the significance of reactions to various picture categories. The assumption is that each category has a unique stimulus value, an implicit and unverbalized associated meaning which forms the basis of the subject's affective reactions. These associative stimulus values must be generally constant in order to warrant the use of comparative inference.

Another basic assumption is that the subject makes his picture selections on the basis of personal affective reactions to the associated stimulus qualities. The forced-choice structure of the test defines individuality in terms of particular selection from limited alternatives. The existence of any cultural bias in favor of certain responses will tend to produce stereotyped affective reactions rather than choices of individual significance. Therefore, the pictures within any test set should have essentially equivalent group affective values.

Personality Inference

The essence of validity for any projective technique is the descriptive or predictive accuracy of the interpretive hypotheses derived from the test behavior. The validity of such inferences can only be demonstrated when the hypotheses are stated in a testable form. Are the Szondi hypotheses capable of systematic investigation?

Deri sets forth certain general hypotheses about test behavior. Frequency of selection within a given category of pictures is considered a measure of the extent of tension within the need-system represented. Thus, the absence of selections in a given category indicates an absence of need tension due to some immediate release, or a more enduring tendency to satisfy the need behaviorally. Conversely, a high frequency of selections within a category indicates an existing tension within the need-system. The role of this need-tension in the personality will be reflected in the manner of selection. Predominant "like" selection within a category is due to some process of identification with the need, or acceptance of it. Predominant "dislike" selection is then due to some kind of counteridentification or rejection of the particular need. The presence of both "like" and "dislike" choices within a category represents internal conflict in regard to the need.

The foregoing hypotheses apply to the interpretation of a single profile and form the basis for the more important interpretive emphasis upon the consistency or variability of choice patterns within a series of administrations. The Szondi Test is unique in this focus upon the personality as a process. Categories with unstable selection patterns represent the more unstable areas of personality in which symptom formation is to be expected. Conversely, consistency of test behavior is to be expected only in those categories which are representative of the stable, core or basic need-systems within the subject. The test is thus not amenable to the traditional measures of reliability, but requires rather the systematic study of unreliability.

The body of interpretive hypotheses for a given technique is an attempt to relate the particular test behavior to more generalized concepts of behavior or personality. The

schema set forth by Deri presents certain difficulties in this regard. The interpretive hypotheses are couched in the language of depth psychology without specification of the phenotypical referents of the concepts involved. Knowledge of their relationships to observable behavior appears essential to validation. Herein lies the clue to that slippery, evasive argument of pragmatic validation that the clinician sometimes employs, i.e., "it works." The vague, ambiguous, and generalized nature of the Szondi hypotheses results in all things being true of all people. The interpreter is often left with propositions having multiple application rather than idiosyncratic reference. The very plastic nature of case material will tend to support such generalized statements. Of course, there is the argument that individual interpretation is actually a process of ordering data in terms of known classes and that, therefore, the concept of a "unique" personality is really a myth. Nonetheless, a technique which purports to describe individuality must describe the individual in terms amenable to validation by external criteria. The need for an explicit statement of the relationships of hypothesized dynamics to overt behavior in given situations has been well stated by Ainsworth in her recent discussion of the validation problems of projective techniques (1).

Review of Research

The present review is intended as an evaluation of the Szondi Test in terms of the studies purportedly investigating either the basic rationale or the interpretative hypotheses of the technique. For a comprehensive bibliography, including expository descriptions and case illustrations, the reader is referred to David (12).

Validation of any instrument for personality assessment such as the Szondi may be approached in essentially two ways: (*a*) A trial-and-error method of empirical investigation can be employed, whereby the body of interpretive hypotheses is gradually verified, altered, or rejected.[2] Obviously a tortuous process, requiring patient exponents and critics, this method has the inherent pitfall of convenient memory. The "correct" hypotheses are readily reinforced, but the "incorrect" ones tend to die a lingering death without any adequate diagnosis of the ailment. This approach has characterized much of the research to date on projective techniques. (*b*) The problem can be attacked at its source, namely, the adequacy of the basic rationale underlying the body of interpretative hypotheses. Note that this approach does not eliminate the necessity for empirical validation, but is concerned with the prior question of whether the technique is actually capable of validation. The writers' point of view is that any technique purporting to reflect personality must have hypotheses based upon a demonstrable rationale. Accordingly, those studies dealing with the process of interaction between subjects and the Szondi materials will be considered prior to the studies of clinical validation. In this way any evident limitations of rationale may help to illuminate the results of inquiry into the interpretive hypotheses.

The only previous review of the Szondi literature is a brief survey by Guertin and McMahan (22). All research covered in that review is considered in the present appraisal.

[2] Sometimes referred to as the method of successive clinical predictions (33).

Studies of Basic Rationale

Investigations of the mediational assumptions inherent in the Szondi Test have dealt mainly with two central problems: the adequacy of the test pictures for communication of personality characteristics, and the assumption that picture selection is a function of individuality.

Content values of the test pictures. As previously noted, a number of subassumptions are involved in the central mediational assumption of the technique, which is that the subject reacts to the personality of psychiatric patients through the medium of facial photographs.

Fosberg (**17**) and Rabin (**31, 32**) have studied one aspect of this problem, the identification of diagnoses from the pictures. The results are in essential agreement and are best illustrated by Rabin. He found that both untrained and trained observers did significantly better than chance, with the latter group superior. Naive subjects tended to improve with increased knowledge of psychiatric disorders. The initial significance and subsequent improvement are both due largely to correct identification of the homosexual and manic pictures. Rabin concludes that physiognomic clues are important, that there is a need to calibrate the observer's ability, and that the findings tend to support the Szondi pictures as meaningful stimuli.

Identification by the use of diagnostic labels is appropriate as a study of the Szondi rationale of associated meanings only in so far as the dynamics implied by the labels are clearly understood by the observer as the hypothesized ones. The existing semantic confusion, ambiguity, and variability of nosological classifications do not appear to be carefully resolved in the present instances. Rather, the Szondi pictures have here been examined for their appropriateness to the observer's unknown nosological frame of reference.

Szondi reports that the communicability of the particular test pictures was established by means of free associations to a wide sampling of pictures for each category. Those producing responses most appropriate to the category rationale were then selected for the test. Deri illustrates this procedure somewhat loosely in her chapter on "Experiment in Factorial Association" (**15**, pp. 17–24). Unfortunately, she gives no indication of any control for rater bias.

Wallen (**44**) has studied the stimulus values of the catatonic (*k*) and paranoid (*p*) pictures by having subjects select the most appropriate characteristics for each picture from a list of thirteen trait pairs. He found no difference between the two categories as to the proportion of votes given to any trait, but there was some suggestion that *k*'s are seen as more "tense" and "inferior," and less "dominant" than *p*'s. The pictures were reclassified for further analysis according to muscular tension. Those perceived as contracted were described as more "self-centered" and the relaxed ones as more "kind" and "optimistic." The latter group of pictures showed greater "like" selection in a related study. Wallen concludes that the Szondi Test requires a frame of reference based upon learned approach-avoidance tendencies in regard to given facial characteristics. This study cuts to the core of the problem of appropriate stimulus values, although it is somewhat limited by the various meanings which may be assigned to trait names by different judges. However, the results do question the adequacy

of the assumption that all pictures within a category have a given meaning.

Klopfer and Borstelmann (**27**) repeated Szondi's experiment of free association to the pictures and supplemented this procedure by matching the pictures to personality descriptions for each of the eight diagnostic categories. Interjudge agreement was 80 to 90 per cent for rating of the associations in terms of appropriateness to the eight dimensions of the test. Subsequent panel decisions resulted in associative valences[3] which agreed with those derived from matchings by subjects in 41 out of 48 pictures. However, only 22 of these valences were those hypothesized by Szondi. The empirically designated "homosexual" and "manic" pictures showed very high agreement with those designated by Szondi, while the epileptic pictures agreed very poorly (out of nine empirically derived valences, only one picture was actually an epileptic). The investigators concluded that the present stimulus materials of the test do not satisfy the basic assumption of appropriate communality of meaning within categories. A redesignation of the pictures in accordance with empirically demonstrated association values was suggested.

There has been much discussion and some study of the bases of picture selection. Some have suggested that the perceived nationality, dress, facial expression, etc. may influence picture choices. This is in contrast with the Szondi-Deri view that the category of mental disease is the determining factor in picture selection. David (**11**) questioned his subjects (paranoid schizophrenics and student nurses) about the identity and nationality of the pictured patients. He found that the psychotics were more prone to accept the pictures as familiar personages than were the nurses. Both David's procedure and his discussion of results fail to deal definitively with his stated problem of peripheral versus dynamic bases of selection. The findings of this study do not indicate just what factors actually determined the selections of the subjects.

Blessing, *et al.* (**3**) have investigated the specific effects of clothing and background characteristics of the Szondi pictures upon affective selection. They found that elimination of all photographic elements except the head produced no change in selection patterns for group administration to college students, but resulted in significant group changes for individual administration to psychiatric patients. This study is an excellent example of the confused and nondefinitive experimental design that is frequent among studies of projective techniques. Different subjects are used under different conditions with no explanation or attempt to reconcile these uncontrolled elements. An explanation of the obtained results in terms of clothing differences among the categories completely misses the main problem—the meaning that these characteristics convey to the subjects. The assumption that the stimulus values hypothesized by Szondi are valid is gratuitous.

In summary, there is positive evidence that some of the test pictures do communicate the qualities hypothesized by Deri, especially in the homosexual and manic categories. The specific elements of mediation are unknown. Only two of the eight Szondi categories seem to have the stimulus values appropriate to the

[3] Associative valence is a term used to refer to the meaningful cluster of associative elements which are produced by a specified population in response to a given picture or group of pictures.

rationale. The failure of the remaining categories to communicate appropriately raises serious questions regarding the use of the test as presently constituted. The suggestion has been made that materials may be restructured in terms of empirically demonstrated stimulus values. Thus, the crucial point is whether subjects in general respond to a given picture as of manic quality, not whether the patient pictured is actually a manic or not. Finally, there is some evidence that such elements as facial muscular tension may contribute to particular associated meanings.

Picture selection and individuality. In addition to the assumption of appropriate content values there is the assumption that picture selections are due to individual reactions to the content values. Harrower's (**23**) collection of some 1300 profiles from neurotic and nonclinical subjects indicates the existence of popular selections, most of them common to both types of subject. Such stereotypes of selection must be explained either by positing a communality of need structure within the group or by attributing the choices to culturally biased reactions to certain physiognomic or pictorial characteristics. In order that the test provide a maximal opportunity for the expression of individuality within the group, the alternatives for selection should have essentially equivalent group affective values.

Since interpretation is based upon reactions to categories of pictures, all pictures of a given category are assumed to have equivalent selective significance. Guertin (**19**) found that selections from a single set were highly reliable, but reactions to the same category in different sets showed no better than chance agreement. He concludes that the subjects respond to specific pictures rather than to categories as presently hypothesized.

The inadequacy of the assumption of intracategory equivalence was further demonstrated by Guertin (**21**) in a factor analysis of preference ratings for two pictures from each category. The resulting factors showed no relationship to test categories, indicating that none of the category pairs showed consistent preferential reactions.

An interesting approach to the assumption of intracategory equivalence has been employed by Lubin and Malloy (**28**). They hypothesized that selections of pictures of the same category should show more positive relationships than pictures from different categories. Single protocols were obtained from 100 psychiatric patients. All possible pairs of pictures from each category were compared in a chi-square analysis of like, dislike, or not selected. No category had all members positively related, while all had certain positive pair relations, although no more than might be expected by chance. The conclusion was that the assumption of intracategory equivalence is not tenable.

The problem of affective variability of the stimuli has also turned up in studies by Guertin (**20**) and Davidson, *et al.* (**14**) comparing selections of Szondi pictures with culturally more familiar pictures. Guertin found that roughly comparable United States pictures evidenced variability equivalent to the Szondi pictures when independently ranked as to preference. He concludes that some of the stimuli are diagnostically better than others since they produce greater variability. Davidson, *et al.* report similar results, demonstrating certain popular reactions for both Szondi and control pictures.

Variability of preference ranking would seem to be a desirable but not conclusive criterion for stimulus structure. Equivalent central tendencies are also essential. The essential structural requirement is equipotentiality *within each set*, not equivalence of all test pictures since each test set is given separately. Therefore, only intraset comparison will answer this question. The contrast with non-Szondi pictures seems irrelevant unless associative stimulus values are considered. Guertin's "control" pictures actually show much less variability when compared directly with the Szondi pictures. However, Guertin's rankings of the Szondi pictures do provide a basis for restructuring of stimulus materials since the present arrangement proves inadequate.

The two most definitive studies of affective stimulus values are reported by Szollosi, *et al.* (**39**) and Borstelmann and Klopfer (**5**). The Szollosi group analyzed single profiles from a heterogeneous sampling of the normal, i.e., nonclinical, population for individual picture reaction patterns. They found that the six pictures of any given test category were not similar enough in affective selection to justify the assumption of equivalence among category members. The limitations of a forced choice situation where the stimuli are of unequal strength are pointed out in that interpretive inferences are demanded even though the individual reactions are equivalent to group stereotypes. They conclude that the stimulus materials should either be restructured for category interpretations or else used for reactions to individual pictures.

Borstelmann and Klopfer demonstrated that individual picture reaction values are not a function of some inherent stimulus quality (as assumed by Deri) but are actually relative values dependent upon the stimulus configuration of a particular test set. Affective ratings of the 48 individual test pictures by 400 college students revealed no consistency in median affective values within test sets or among members of any category. The test sets were then reconstituted so as to more nearly approach equivalence of affective stimulus values. Thus, the most liked pictures of each category were assigned to one set, the most disliked to another set, etc. The popular trends of selection were significantly reduced with the reorganized test materials. However, categories still differed markedly from each other in the general level of affective pull.

This raises a fundamental, and possibly insoluble, problem in the use of the forced-choice technique for the Szondi pictures. Szollosi, *et al.* infer that discrepancy in preference values indicates differential picture qualities. Wallen found that distinguishable stimulus qualities were related to divergent affective values. Therefore, existing evidence suggests that the very stimulus qualities that produce distinguishable associations also produce divergent affective reactions. If so, then the forced-choice structure will not reflect the desired individuality of response, since the alternatives for selection are unequally biased. Of course, both the Wallen and the Borstelmann and Klopfer findings are limited to the circumscribed population of college students. But Harrower's studies with noncollege groups show similar selection stereotypes. Even if the results should subsequently prove to be due to the peculiar need structure of college students as a group, the detection of individuality within the group is still questionable. Thus the present test structure would

yield only group, not individual, identification.

In summary, the necessary structural requirements of the Szondi forced-choice technique as a device for the reflection of individual uniqueness do not seem to be adequately fulfilled at present, nor even capable of fulfillment. Such a framework assumes that selection of pictures is due to the particular need structure of the individual. To the extent that responses are determined by group bias, the expression of individuality is necessarily restricted. The presence of persistent "picture pulls" within the present test structure has been clearly demonstrated. Furthermore, the establishment of equivalent selective significance among the test categories seems highly improbable since associative and affective qualities are not independent. Thus, pictures of demonstrated "manic" quality are generally liked while those of "sadistic" quality have generally negative values.

Studies of Interpretive Hypotheses

The studies reviewed above have not been concerned with interpretive hypotheses, but only with the question of whether the technique is capable of validation. With the limitations and deficiencies of the Szondi rationale now set forth, the studies dealing with interpretive hypotheses may be more rigorously appraised.

Normative studies. Deri makes many statements of genetic and socioeconomic trends as illustrations of interpretive hypotheses. A number of studies have reported picture selection patterns for various groups. Hill (25), Guertin (18), and Fosberg (17) have analyzed Szondi Test behavior in terms of deviations from chance phenomena. Cohen (7) provides an excellent didactic presentation of the chance probabilities for each type of category selection (plus, minus, etc.) with any given frequency of choices for the category.

Guertin compared profiles of hospital attendants with those derived by random numbers. He found no difference in frequency of category choice or in positive selection, more negative selection and intracategory imbalance for the subjects, and more ambivalent selection within categories for the chance profiles. He concludes that frequency of selection is not indicative of need-tension, that ambivalence is indicative of stability rather than conflict, and that negative selection is most reflective of tension. The hypothesis under investigation here is that different states of underlying need-tensions are reflected in differential picture selection patterns. Guertin's study invokes a hidden assumption of the existence of the appropriate need-tensions, since there is no external criteria of such tensions. Further, there is no indication of the extent of homogeneity or heterogeneity of the group in regard to the variables under consideration. Therefore, the only justifiable conclusions would seem to be that hospital attendants can be characterized by certain negative and unbalanced category selections. Whether these characteristics are peculiar to the attendant population is unknown.

Fosberg compared category "like" and "dislike" patterns for a normal and a patient group with chance probabilities.[4] The results indicated that both groups differed significantly from chance selection but not *from each other*. The conclusion is drawn that the psychological factors at work in the test behavior are not

[4] Fosberg's miscalculation of probabilities has been pointed out by Cohen in a recent note (8).

those hypothesized by Deri and Szondi. As with the Guertin study, the existence of certain need-tensions within the subjects is assumed without any supportive evidence. Therefore, the results of a comparison with chance are difficult to interpret. On the other hand, the test's inability to differentiate between patient and nonclinical groups would seem to discredit the Szondi hypotheses. However, Fosberg does not specify how many or which categories should hypothetically distinguish the groups on the basis of single profiles. The main hypothesis of pathology regarding fluctuations in certain categories has not been considered. Thus, normal and pathological groups might evidence significant differences in stability for a series of profiles without any single profile distinctions.

Hill compared category, vector, and individual picture selection patterns of 1,066 college students with chance probabilities of such patterns. He found that most choice patterns differed significantly from chance. However, he points out that failure of chance to account for the test phenomena does not provide any information as to what psychological forces are actually operative. Deri's concept of need-tension remains neither verified nor rejected. Hill did find that males and females differed significantly in selections of sexual (homosexual and sadistic categories) and cyclic (depressive and manic categories) pictures, but the source of these differences is not known.

The most extensive normative data reported to date are in Harrower's (**23**) report of single profiles for some 1300 normals and neurotics. She records the distributions of the various selection patterns for each of several designated subgroups. Harrower merely states results without any discussion of pertinent Szondi hypotheses.

Deri makes repeated reference to certain syndromes of category selection with the implication of correlation of choices between the pertinent categories. Borstelmann (**4**) analyzed single profiles of 400 college students using a group administration, and found no evidence of covariation between any pairs of categories hypothesized as having a consistent relationship. Thus, the occurrence of a given combination of categories is no greater than would be expected by chance. There would seem to be no justification for the inference of common elements among certain categories. Borstelmann's results further indicated no sexual differences in choice patterns; a predominantly positive selection of paranoid and manic pictures; a predominantly negative reaction to hysteric, catatonic, and depressive pictures; and more variable selection of homosexual, sadist, and epileptic pictures. Although some support was evident for Deri's characterization of a young adult population, the choice patterns in the paranoid and depressive categories were opposite from those hypothesized.

Cole and Roberts (**10**) gave a group administration of the test to 50 students of introductory psychology at ten consecutive class sessions. Most of the pictures elicited a general positive or negative, rather than variable, reaction from the group. There was no consistency in group reaction among category members. There was some evidence of sexual differences and support for Deri's statements of trends. There is unfortunate confusion here between group and individual trends, as well as between stimulus and response consistency. Cole has not provided the crucial analysis of his data, the consistency

or fluctuation of group and/or individual responses to the test materials. Consideration of the data as 500 single profiles tends to obscure the behavior most meaningful to Deri's hypotheses regarding temporal effects.

Helme (**24**) did a factor analysis of category intercorrelations for single profiles of 72 young adults. Category selections were expressed in terms of "directive scores" (algebraic sum of plus and minus choices, regardless of loading). A simple cluster analysis resulted in "a major cluster for both males and females, comprising the sexual (homosexual and sadist) and ego (catatonic and paranoid) categories and the hysteric category attached to the sexual categories" (p. 5). This and other clusters are interpreted in accordance with Deri's hypotheses. Note that Helme, in contrast to Borstelmann, found both category intercorrelation and sexual differences. The contradictory evidence may be a function of the method of analysis. Borstelmann used a chi-square test of covariation for the four main scores of Deri (plus, minus, ambivalent, open). Helme's algebraic sums offer finer distinctions of category selection but at the same time combine in a single score several of the patterns used by Deri. This extended scoring schema may offer potentially more meaning for test interpretation, but it is not appropriate to Deri's hypotheses.

Finally Spitz (**38**) briefly reports on some of the results from a study of 110 Swiss kindergarten children. Although she notes that the children's choices are strongly affected by such peripheral factors as beards, the validity of dynamic sources for picture selection are taken for granted. Thus, test results differing from Szondi's results with Hungarian children are interpreted as personality-cultural variations. There is no apparent recognition that the stimulus properties of the pictures may differ for the two groups.

In summary, the evidence is rather clear that chance does not account for Szondi Test behavior but the nature of the psychological forces at work remains somewhat obscure. The accumulated results of test behavior for various groups are difficult to appraise owing to a lack of specification of the relationships between the test variables and known characteristics of the groups. Use of the test hypotheses to explain the test results does not provide critical evidence for the validity of these hypotheses. The method of empirical groups as a test of hypotheses would seem to have value only to the extent that the groups are clearly differentiated in regard to pertinent characteristics. Unspecified overlap between the groups will tend to obscure the relationships between variables. Selection of subjects who have known characteristics clearly relevant to specified test variables would seem to offer a more definitive approach to the investigation of interpretive hypotheses.

Stability and variability of test behavior. As previously indicated, the traditional correlational concept of psychometric consistency is not applicable to phenomena involving expectations of change or variability. Deri hypothesizes that the extent and location of change in test behavior are reflections of intrasubject variability.

Cole (**9**) compared initial and subsequent protocols for 86 college students. Seventy-four subjects had change of direction for two to five of the eight test categories. The changes were mostly of a minor nature, with few actual reversals of direction within a category. No cate-

gory was more changeable or stable than the others. Cole merely concludes that a single Szondi profile is not reliable. However, these results tend to support the proposition that the test does not reflect such relatively stable functions as cognitive processes and is, therefore, not reliable in the traditional sense of the word. The important question is whether the observed changes in test behavior are meaningfully reflective of subject functions or merely fortuitous.

Deri hypothesizes that fluctuations in test behavior are due to intrasubject instability, greater variability being generally more indicative of pathology. David and Rabinowitz (**13**) gave a series of six individual administrations to 20 paranoid schizophrenics and 20 student nurses. A Szondi Instability Score was computed on the basis of successive reactions (like, dislike, ignore) to individual pictures. The patient group had significantly greater variability, but not for any specific category.

Although this study represents a limited sampling, it seems that the results tend to support the general hypothesis of the relationship between pathology and instability of test performance. However, the hypothesis of greater instability of schizophrenics in the ego vector (catatonic and paranoid categories) is not borne out. The greater variability of psychotics' test behavior may be attributed to a general instability of functioning rather than to the specific dynamics hypothesized by Deri. David's method of analyzing reaction changes to specific pictures is particularly profitable because it avoids the unwarranted assumption of intracategory equivalence of stimuli. Thus, test change may be appraised in terms of any stimulus groupings that have demonstrable significance. It would be interesting to analyze the results further. Is the group difference due to more variable reactions to certain pictures? Is there any consistency of reaction to pictures within existing categories?

The relation of test change to change in overt behavior has been carefully studied by Paine (**30**). Six test profiles given at two-day intervals to 30 functional psychotics were compared with daily ratings by attendants on a 15-item behavior scale. Test change was expressed as differences in choices for each category, divided into five gradients of change comparable to Deri's classes. There were no relationships between behavior and test changes. Subjects with the greatest test change were not consistently more unstable or severe in pathology. Open reactions were not related to overt behavior. The open-ambivalent ratio for the test series was not indicative of either pathology or control. Finally, the extent and kinds of change were not different from an independent group of college students.

These completely negative results must be due to either inappropriate study design or inadequacies of basic hypotheses. If one argues that the overt behavior observed herein is not relevant to the personality characteristics reflected in the test, then one is bound to specify more adequately the phenotypical referents of the hypothesized variables. Meanwhile, the results must be interpreted as casting some doubt on existing hypotheses about the test.

An interesting approach to a systematic investigation of unreliability has been suggested by Holt's (**26**) study of intra-individual covariation for a single subject in twelve administrations of the Szondi Test, Murray's "Mind-Reading" test, and self-ratings on a 130-item question-

naire. Numerous "significant" correlations (.70 and greater) between fluctuations are reported. The pertinence of such methodology to the intricate problem of reliability and validity in personality measurement is discussed by Holt. However, in the absence of more precise specification of the relation of test behavior to the total functioning of the individual, evidence of covariation among assessment techniques will constitute only presumptive validity of the test hypotheses.

Cahill (6) infers from Deri's exposition a hypothesis of greater intra-individual complexity and hence more variable test behavior for individuals of higher intelligence. He found no differences between high school students of low average and superior IQ's. Test change (five administrations) was expressed in terms of any change of choice patterns as a percentage of total possible change with a given category. This method of measuring change in test behavior allows for an element not controlled by other procedures, the actual freedom for alteration which is not equivalent for all patterns (plus, open, ambivalent, etc.). However, the basic hypothesis of the study seems to be a misinterpretation of Deri's position regarding level of intellectual ability. Differences in intelligence lend interpretative qualification to the hypotheses but *are not* postulated as actually producing differences in test patterns. The results actually support Deri's proposition that test behavior is a function of something other than cognitive processes.

In summary, variability seems to be a normal, nonclinical function of Szondi Test behavior. Though not extreme, this variability seems to be pervasive and not differential among test categories. There is greater, though still undifferentiated, variability among pathological subjects. Whether the changes in test behavior are related to hypothesized variables of intra-individual functioning is unknown; existing evidence is somewhat contradictory and indefinite. Clarification and verification of interpretive hypotheses regarding stability or instability of response will require extensive systematic study of intra-individual variability in terms of carefully specified relationships between test behavior and other functioning of the individual.

Experimental change. The studies discussed in the preceding section were concerned with the normal or usual fluctuations in test performance as reflections of intra-individual variability. Further clarification of the meaningfulness of variability or stability in test behavior may be elicited by use of the experimental situation method. The essential question is the kind and extent of test change that will result from the interpolation of specified independent variables.

Scherer (36) studied the effects of administration procedure by testing 22 mental patients with a battery of instruments, including the Szondi, in both individual and group situations. Certain significant differences in Szondi profiles were obtained and interpreted in terms of Deri's hypotheses. In this study, the lack of a priori specification of expected differences necessitates a posteriori speculation as to the psychological significance of the obtained results. Such a procedure necessarily involves the questionable presumption that the hypotheses are valid. The results can therefore be considered only as suggestive for further research.

Another study by Scherer (37) was

designed to assess the effects of the examiner upon subject behavior. Five paranoid schizophrenics were tested by five different examiners on successive days, and each examiner self-administered the test immediately following the subject's test. Five similar subjects were tested for five days with a group administration and asked to match each picture to one of eight descriptive phrases representing the test categories. The results showed that group profiles differed from individual administration only in greater plus choices for paranoid pictures. There was some influence (unspecified) of the examiner's personality. No significant daily variation was found in either patients or examiners. Some unique as well as common individual picture patterns for patients and examiners were evidenced.

This ambitious study design is an excellent example of attempting to do too much with too little. Examiner effects upon test behavior is an important area for study, and variation of examiners would seem to offer an appropriate method. But use of the test itself as a measure of the examiner is rather like pulling one's self up by one's own bootstrap. The presumption that the examiner-subject interaction will be reflected in the instrument being studied is gratuitous. Experimentation requires investigation of, not defense of, the hypotheses being studied. Scherer's approach should be encouraged under conditions of external criteria for both subjects and examiners.

Saunders and North (**34**) have tested the hypothesis that test behavior should not be sensitive to transitory sets if the behavior is due to basic personality structure. Four hundred and twenty students were divided into three groups with each receiving two standard administrations and an interpolated test of altered instructions. In the interpolated tests Group A was given the standard instructions of like-dislike selection, Group B was instructed to make happy-unhappy choices, and Group C was instructed to make strong versus weak character selections. Three of the six tests sets were used for the pre- and posttests, with the remaining three used for the interpolated test. The extent of agreement between pre- and posttest selections was essentially equivalent for all three groups, a mean of 75 per cent duplicate reactions. Thus, the test behavior does not seem to be affected by a transitory alteration of set toward supposedly equivalent stimulus materials. However, it should be noted that the assumption of equivalent stimulus values from set to set has not been upheld. In the limited sense that test-retest agreement exceeds chance expectations, the test may be said to be reliably assessing whatever forces are operative in the test behavior.

Odes's study of the effects of exposure to a supposedly strong emotional stimulus (**29**) is indicative of the inadequacies of experimental control that tend to confuse investigation of projective techniques. Forty graduate students were given two administrations in a single session with interpolated reading of either a short horror or humorous story. A statement is made by Odes that "a majority of the critical subjects had strong emotional reactions to the (horror) story and gave notable changes in their Szondi profiles after the story" (p. 3). But there is no indication of the criterion of "strong emotional reactions." Apparently there was no attempt to control contamination between assessment of the independent and de-

pendent responses, judgments of emotional reactions and interpretations of test behavior being made by the same judge.

Studies by Deri (16) and Fosberg (17) have investigated the effects of electroshock therapy upon Szondi Test behavior. Deri hypothesized that the tendency of depressive patients to reject sadist pictures represents an accumulated introverted aggression which would be released by shock treatment, thereby altering the selection pattern of sadist pictures toward less rejection. The results show that nineteen shock patients tended to have less sadist rejection and increased rejection of catatonic pictures, while ten nonshock patients and ten hospital employees did not. The increase in catatonic rejection is explained as greater repression concomitant with less introverted aggression.

This study is a good illustration of apparently significant results and consequent evidence of validity. However, the results merely constitute presumptive validity. Note that there is no independent evidence that either the depressive syndrome or the rejection of sadist pictures actually represents "introverted aggression." Further, the comparability of the control subjects is based only upon the mean algebraic sums of choices for each factor, with no indication of comparable factor choice frequencies or the extent of individual variability within the groups. Finally, other apparently significant differences are not accounted for. Thus, although Deri makes much empirically of the increased rejection of catatonic pictures by the shock subjects, there is no explanation of why the patient controls already have such a pattern without any shock treatment.

Fosberg tested a hypothesis of Szondi's that the need for emotional control represented in paroxysmal vector (epileptic and hysteric pictures) choices will be decreased following electroshock seizure. Twenty-five male patients, serving as their own controls, were tested after recovery from seizure and also when no seizure had occurred for three days. There were no differences in choice patterns for epileptic, hysteric, or sadist pictures. Since this study represents a direct test of one of Szondi's hypotheses, the negative results must be accounted for either by inadequacies of hypothetical rationale regarding the significance of test behavior, or inadequacies of postulated interrelationships among the hypothesized variables. As studies of rationale have indicated that the stimulus values of epileptic and hysteric pictures are not pertinent to the hypotheses, the present hypothesis should be investigated with stimuli of appropriate values.

Fosberg also studied the hypothesis that overt discharge or release of sexual tension will be reflected in decreased selection of sexual (homosexual and sadist) pictures. Ten male and ten female subjects volunteered to take the test and tell the examiner when their last sex episode occurred. There were no differences in homosexual and sadist picture selections between reported overt sexual activity within twelve hours or over forty-eight hours. Fosberg has suggested a pertinent design for the relationship hypothesized by Deri between test behavior and overt activity. Of course, the cooperative declarations of the subjects may be a reflection of more conscious awareness and perhaps less nonconscious tension regarding sexual needs.

In summary, there are some indica-

tions that Szondi Test behavior may be affected by differences in examiners and conditions of administration, though the meaning or significance of such effects remains unspecified. Whatever psychological forces are operative in the test behavior, performance seems to be relatively consistent in so far as it is not contaminated by alteration of set toward similar test materials. The evidence of change in test behavior due to some altered state of the organism is either inconclusive or negative. The consideration of priority of investigation in validation procedures should again be noted. Critical tests of interpretive hypotheses must be phrased in terms of logical relationships based upon demonstrated qualities and phenomonology of stimulus-subject interaction. The validity of a hypothesis cannot be ascertained unless it is capable of validation.

Discussion

Theoretical and Methodological Considerations

The Szondi Test, in common with all techniques which purport to reflect the operation of personality functions, makes certain basic assumptions:

1. All behavior is assumed to be meaningful and not fortuitous.

2. The subject's test behavior has implications for generalized inference, provided the nature and extent of such inference can be adequately specified.

3. The human organism is internally consistent in its behavior. All behaviors are not necessarily identical, but they are related in comprehensible ways to the total economy of the organism. Thus, apparent inconsistencies in behavior are assumed to be due to inadequate comprehension of the observer.

4. Behavior involves different levels of the personality. To some extent the subject's behavior is due to forces within himself of which he is unaware. Moreover, some aspects of test behavior are assumed to reflect overt behavior tendencies while others reveal more covert characteristics of the individual.

5. Individuality consists of a unique constellation or patterning of dimensions that are common to all persons, not of forces which are peculiar to the individual. The basic phenomenological characteristics of projective test behavior must therefore have a core of generalized inference applicable to all persons making such a response, but capable of qualification or refinement in terms of other phenomena. The more complex the interrelationships of phenomena, the more individual the significance.

6. The interpretive inferences from a given set of data can be stated in terms of some general conceptual framework of personality. It follows that the interrelationships of various techniques may be made explicit in terms of their mutual or distinct relations to the more generalized concepts.

In addition to the general assumptions of projective techniques, there are certain theoretical assumptions essential to the nature of any given technique. In the case of the Szondi Test these are as follows:

1. Personality may be meaningfully described in terms of differential patterns of eight common need-systems. These "needs" are presumed to provide a significant sampling of the personality characteristics of an individual.

2. A task requiring mainly affective structuring by the subject will

provide sufficient scope to reflect individuality. The Szondi Test requires expression of affective attitudes toward stimuli which are readily identifiable (as contrasted with the Rorschach or even the Thematic Apperception Test). Individuality is presumed to be expressed within the limits of a forced-choice task, freedom of individual response being restricted to affective selection among limited alternatives of recognizable stimuli.

3. Affective selection is assumed to be based upon certain generally associated meanings for given classes of stimuli. A given category of pictures is assumed to have equivalent associative values, yet potentially divergent affective meanings. The patterns of affective selection purportedly reflect individuality in terms of intra-individual complexity rather than as syndromes of deviation from normative patterns.

4. Finally, there is the assumption that behavior in one modality may be inferred from that in another modality. The nonverbal Szondi behavior is presumed to be predictive of the verbal behavior of symbolization and even such motoric behavior as expressive movement.

Three frames of reference have been advanced to explain the phenomena of affective selections among the Szondi pictures. Szondi himself maintained that the eight clinical syndromes pictured in the test represent certain personality characteristics which are hereditarily determined and manifest in physical appearance. Subjects are assumed to react to the test pictures on the basis of their own patterns of recessive or dominant genes. The knowledge of gene structure derived in this way leads to inferences about the subject's personality.

Deri, omitting the genetic assumptions of Szondi, hypothesizes that the clinical syndromes of the test are extreme manifestations of personality characteristics present in all persons. The affective selections by the subject are due to unconscious identification with or rejection of these characteristics within himself.

Both Szondi's and Deri's formulations assume that the dynamics of a given syndrome are reflected in the test pictures and reacted to as such by the subjects. However, only some of the test pictures have been found to have the stimulus values appropriate to such rationale.

The present writers have maintained that the mediational assumptions implicit in the Szondi and Deri formulations are not essential to the use of the test. A more empirical frame of reference in regard to the test phenomena is proposed. Like any other projective technique, the Szondi Test may be considered as consisting of partially structured stimulus materials which the subject is asked to handle in his own personal manner, thereby revealing something of his unique personality processes. The subject expresses affective attitudes towards the pictures in a direct manner by selection as like, dislike, or relatively neutral. These affective choices would seem to be based upon implicit and unverbalized associations to the stimuli. Inferences about individual characteristics would thus seem to be justified if the following necessary and sufficient assumptions maintain: (*a*) Subjects tend to assign consistent, though unverbalized associations to the pictures. (*b*) In selecting among the pictures, the subject responds in terms of personal affective, rather than stereotyped cultural, values. (*c*) Reaction tendencies toward pictures of common associative

values have interpretive significance which can be demonstrated by external criteria.

Within this empirical frame of reference as differentiated from Szondi and Deri, the test pictures may be restructured in accordance with demonstrated stimulus values. Any tenuous assumptions about manifest physical expression of personality are therefore unnecessary. With pictures of stimulus values appropriate to the test rationale, hypotheses relating to particular reaction patterns may be definitively investigated. Without a demonstrable rationale, the validity of the interpretive hypotheses will remain uncertain.

The Validity of Interpretative Hypotheses

The body of interpretive hypotheses for such techniques as the Rorschach, Thematic Apperception and Szondi tests are designed to assess individuality of functioning as reflected in the pattern of responses to a standard set of stimuli. The methods of analysis or evaluation (scores, abstracted themas, etc.) provide a means for ordering the unified personal productions of the subject in terms of the interpreter's conceptual and experiential framework. In this sense the "gestalt" of the subject's performance is somewhat arbitrarily destroyed and distorted in terms of the interpreter's orientation. Thus, the separate hypotheses formulated do not represent distinct and independent bits of response to be combined in a purely summative manner. Each hypothesis must have a core of generalized inference which is valid for all subjects so responding, yet capable of qualification in terms of relationships to other hypotheses. Here lies the seat of much confusion about validation of isolated hypotheses. Only the generalized hypothesis can be investigated by group differences. The qualifications, in terms of related hypotheses, require increasingly idiographic verification. Behavior must be studied in the context in which it is exhibited, not as it suits the convenience of the investigator.

An illustration of investigation of the validity of generalized inference from a single aspect of test phenomena is provided in an unpublished study of Bardsley, Borstelmann, and Klopfer (**2**). The hypothesis under investigation was the inference of differential handling of passive needs from different reaction patterns to "homosexual" pictures of the Szondi test. The selection of stimuli as adequate for representation of such needs was based upon the Klopfer-Borstelmann study of associative stimulus values (**27**). The stimulus pictures were presented in rearranged groups so that each "homosexual" picture had relatively equivalent potential for selection (see Borstelmann-Klopfer [**5**]). Subjects were given a standard administration of the restructured Szondi test, as well as various other techniques and interviews specifically designed to elicit evidence of the functioning of passive needs. The results then provided a basis for ascertaining whether differential responses in particular Szondi behavior were similarly reflected in external criteria of the postulated variable. The point intended to be stressed here is not the end result of verification or refutation of the particular hypothesis, but the adequacy of methodology for validation of any interpretive hypothesis. The present writers maintain that any body of interpretive hypotheses must be capable of systematic verification.

A plea for scientific rigor in the validation of projective techniques inevitably raises the ghost of "clinical intuition." Some clarification of this factor in the interpretive process seems appropriate. The present writers maintain that intuition is not a function peculiar to the clinician. Rather, intuition would seem to refer to subliminal and implicit hypotheses that the investigator (clinical or experimental) has acquired in the course of repeated exposure to given phenomena. In so far as clinical intuition refers to recognition of the complex, interactive nature of individual behavior, it serves an important function. As a defense against explicit systematic investigation, it seems completely untenable. The progress of knowledge is dependent upon adequate communication. What seems to be called for is a more explicit operational statement of the implicit hypotheses used by the clinician. Systematic investigation of the clinician in the process of interpretation should be encouraged.

The question naturally arises as to whether such a technique as the Szondi Test is worthy of the rigorous investigations essential to determination of validity. Certain unique contributions of such a technique should be noted. The very structure of the forced-choice task which restricts the freedom of individual expression also yields a desirable objectivity of administration and scoring. Although interpretation remains a tenuous process of undetermined validity, the data forming the basis of interpretive inference can be established with a high degree of consistency among observers. The simplicity of the task required of the subject permits a wide range of application, from children to old age, feebleminded to superior mentality, normal to severe pathology, etc. Finally, the technique is unique in the use of a temporal series of behavior samples. Greatest interpretive emphasis is placed upon indices of change or stability. As such the test seems peculiarly well suited for the study of personality processes. If the Szondi Test could be put on a sound theoretical and empirical footing, it would seem to be a valuable addition to clinical and research tools.

Perhaps indicative of a favorable prognosis for projective techniques is the absence of studies dealing solely with the ability of the Szondi Test to discriminate among diagnostic groups. The inadequacies of nosological groups as external criteria for instruments that are ***not*** designed primarily to make such group differentiations have been repeatedly stressed by recent writers. Yet the literature concerning the Rorschach and other projective techniques abounds with studies of this kind. The present writers are encouraged to believe that research in the area of clinical assessment has begun to evidence sophistication in regard to methodology and study design pertinent to investigation of the assumptions underlying multidimensional techniques. To be sure, the present review has found the existing status of research with such an instrument as the Szondi Test far from being a paragon of experimental virtue. Yet the focus upon the adequacy of the rationale involved in the nature of the Szondi Test is indicative of a trend toward more fundamental and definitive inquiry. Any clinical instrument is not a shibboleth to be defended or refuted, but a provocative tool which requires careful investigation as to the nature of its contribution to the understanding of individual behavior.

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SOME RELATIONS BETWEEN TWO STATISTICAL APPROACHES TO ACCIDENT PRONENESS[1]

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In the pursuit of the elusive concept of "accident proneness" a number of statistical techniques have been used. Recently, several articles on the relative merits of two of the more widely used techniques, the correlational method and the "Poisson fit" method, have appeared (**1, 2, 5, 6**). The present article is about an apparently basic identity between these methods. From our analysis it appears that a tempest may be brewing in the teapot.

The "Poisson fit" technique was developed by Newbold in 1927 (**7**) and by Cobb, independently, in 1940 (**3**). This method is an extension of the method of simply comparing an obtained distribution of accident occurrences of indivuduals with a Poisson distribution. Such a comparison can indicate the presence of "other than chance" factors but gives limited information about the extent of the "other than chance" factors. The Newbold and Cobb attempt to correct this deficiency is based upon the fact that the mean and the variance of a Poisson distribution are identical. Under certain assumptions, the difference between the mean and the variance of an obtained accident distribution may be taken as representative of the degree of accident proneness in the population from which the distribution was obtained. As stated by Thorndike:

> If we have a series of single distributions, each of which is a Poisson, and wish to combine them into a single overall distribution, the mean of the total distribution will be the weighted average of the means of the sub-distributions . . . for each sub-group, the variance around its own mean is equal to that mean. The weighted average of the within-groups variance will, therefore, equal the weighted average of the sub-group means, i.e., the mean for the total group. Having this estimate of the within-groups variance, one can compute the total variance for the empirical distribution, and then one can immediately estimate the between-groups variance by subtraction (**8**, p. 34).

These obtained variances may, in turn, be converted into a more useful form by noting that the between groups variance divided by the total variance is equal to R^2. This statistic in a sense represents the reliability coefficient of the accident criterion. The square root of this result is equivalent to the index of reliability.

Assuming, then, that the subdistributions of our population are Poissons, this seems a legitimate method of estimating the extent to which "other than chance" factors are operative in our population. This method is that preferred by Blum and Mintz (**1**).

The other method that we are concerned with is the correlational method. Its assumptions are straightforward enough. The period in which the subjects are operating is divided into two half periods. It is

[1] The basic notion of the binomial expansion was developed by W. B. W. and he assumes responsibility for the conclusions drawn in this article. The collection of the data was done by E. R. J. The method was developed as a part of an Air Force contract sponsored by the Human Relations Research Laboratory.

assumed that if a differential consistency of accident behavior is operative in the subjects this will result in a correlation between the two periods. That is, the highly prone individual in one period will be highly prone in the other period, the individuals of low proneness will exhibit this characteristic in the two periods, and a correlation of the accident behavior of the individuals between the two periods will reveal the extent of the consistency of accident performance above and beyond factors distributed independently of the individual's performance. This method is preferred by Maritz (**5**).

The two methods described above were applied to an Air Force population in an attempt to determine the degree of "other than chance" factors operative in the Air Force flying situation.[2] The "Poisson fit" method was applied to the distribution given in Table 1. The resultant R^2 was .204.

into accidents occurring on odd days and even day. Fortunately, this was easily accomplished by IBM methods since the cards used contained the day, the month, and the year of occurrence for any given accident.

The resultant correlational table is given in Fig. 1. The product-moment correlation obtained was .107. Corrected by the Spearman-Brown formula, this correlation was .193.

In working with this table it was noted that the diagonal frequencies of the table approximated a binomial distribution for each diagonal. Further consideration led to the conclusion that a binomial expansion of the obtained accident frequencies would represent the best theoretical distribution of these accidents in a bivariate division. It was reasoned as follows: given one accident, the probability of the accident occurring in one or the other of two independently determined periods would be equal.

TABLE 1

DISTRIBUTION OF INDIVIDUAL NONFATAL ACCIDENTS DURING A PERIOD OF AIR FORCE FLYING

Number of Accidents	0	1	2	3	4	5	6	7
Number of Pilots	12475	4117	1016	269	53	14	6	2

The correlational method was also applied to this population. Because of changing assignments and varying circumstances of flying between periods of Air Force flying, the periods were not divided by a first-half–last-half method. Rather, in an attempt to equalize experience, exposure, etc., the data were divided

Given a number of single accidents and two "periods," the most likely distribution of the accidents would be that of half the accidents occurring in one period and half the accidents in the other. Given a sample of individuals having two accidents with equal probability of occurrence in either of two periods, the most likely distribution of these accidents would be the occurrence of one-fourth of both the accidents occurring in one period; one-fourth of both the accidents occurring in the other period; and half the accidents distrib-

[2] The actual time period and circumstances of operation for the population have been purposely omitted for security purposes. This also fortunately limits inferences concerning Air Force accident proneness, which deserves a more detailed statement.

Accidents Occurring on Odd Days—Even Years—Even Days—Odd Years							
6	0	1	0	0	0	0	0
5	0	1	0	0	0	0	0
4	4	4	1	1	0	0	0
3	29	10	5	2	0	0	0
2	234	101	26	4	1	0	0
1	2045	513	100	7	2	0	0
0	12475	2071	269	39	6	0	1
	0	1	2	3	4	5	6

Accidents Occurring on Odd Days—Odd Years—Evens Days—Even Years

FIG. 1. DISTRIBUTION BY PERIOD OF OCCURRENCE OF INDIVIDUAL NONFATAL ACCIDENTS

uting themselves so that one accident occurred in one period and one in the other period. This distribution, of course, is that obtained by a binomial expansion with a .50 probability, taking into account the number of events.

It then was hypothesized that accidents will distribute themselves binomially between two periods if the accidents are independent of the method of division of the periods. Any other distribution would indicate that the method of division was related to the accident performance of the subjects.

Following this assumption, we constructed a correlational table from the originally obtained accident distribution given in Table 1. This distribution is given in Fig. 2. It will be noted that each diagonal is a binomial distribution (the 1-1 coordinates, the 2-2 coordinates, etc.). The obtained correlation was .112. An application of the Spearman-Brown formula resulted in an r of .201. It is interesting to note that this form of distribution is precisely that which was generated by Burke in a separate article on this problem utilizing different assumptions (2).

This correlation looked familiar. It was in fact identical to the second decimal place with the R^2 obtained by the analysis of variance method. To check the possibility that this was a strange coincidence rather than a factual similarity of results for the two methods, a number of other dis-

5	0.4	0.0	0.0	0.0	0.0	0.0
4	3.3	2.0	1.0	1.0	0.0	0.0
3	33.6	13.2	4.0	2.0	1.0	0.0
2	254.0	101.0	20.0	4.0	1.0	0.0
1	2058.5	508.0	101.0	13.2	2.0	0.0
0	12475.0	2058.5	254.0	33.6	3.3	0.4
	0	1	2	3	4	5

FIG. 2. BINOMIAL EXPANSION OF ACCIDENT DISTRIBUTION

tributions were obtained and the two methods were applied. The results in these cases were identical to the third decimal place.

A mathematical analysis of the bivariate binomially derived distribution was performed by Burke. A note by him appears following this article. Essentially his analysis indicates the mathematical identity of the two methods.

Certain conclusions may be drawn:

1. Two methods of estimating the theoretical extent of "accident proneness" may be derived from essentially independent assumptions. One, the analysis of variance technique, makes its assumptions concerning the subdistributions of the obtained data. The other, the binomial correlational method, assumes a chance distribution of accidents for any subject between two periods of operation.

2. Operationally, the two methods yield identical estimates.

3. Mathematically, the identity of the methods may be shown.

4. Theoretically, many of the arguments concerning the relative merits of the two methods must become suspect.

5. Practically, the results indicate that the choice of the two methods outlined becomes dependent only upon convenience, ease of conceptualization, or personal preference.

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NOTES CONCERNING THE WEBB-JONES ARTICLE

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We consider a set of accident data on N individuals. To each individual there corresponds a number z, the number of accidents he has had during the entire observational period. The period is divided into two exclusive and exhaustive periods of equal length; for each individual, x and y represent the number of accidents he has had in each of these periods. Clearly, x, y, and z are discrete random variables and

(1) $$z = x + y.$$

We associate the following distributions with the variables:

(2) $p_{i.}$ (the probability that $x = i$)

(3) $p_{.j}$ (the probability that $y = j$)

(4) p_k (the probability that $z = k$)

(5) p_{ij} (the probability that $x = i$ and $y = j$).

The "binomially-partitioned" distribution introduced by Webb and Jones (**5**) is obtained in the following way. The expected number of individuals who have precisely k accidents during the entire period is given by:

(6) $$n_k = np_k. \qquad (4)$$

For each k, these n_k values of $z = x + y$ are partitioned into specific values of x and y according to the successive terms in the expansion of $(\frac{1}{2}+\frac{1}{2})^k$. Hence, we have:

(7) $$p_{ij} = p_k \binom{k}{i} \left(\frac{1}{2}\right)^k = p_{i+j} \binom{i+j}{i} \left(\frac{1}{2}\right)^{i+j}. \qquad (1)$$

Where $\binom{k}{i}$ is the number of combinations of k objects taken i at a time.

We wish to establish two important facts about the distribution defined by (7).

I. The correlation between x and y, corrected by the Spearman-Brown formula, yields the expression used by Newbold (**4**) and by Cobb (**2**).

For the average value of z, we have:

(8) $$\overline{Z} = \sum_k k p_k. \qquad (4)$$

For the average value of z^2, we have:

(9) $$\overline{Z^2} = \sum_k k^2 p_k. \qquad (4)$$

For the variance of z, we have the well-known expression:

(10) $\sigma^2 = \overline{Z^2} - \overline{Z}^2$. Cramér (**3**, p. 180)

For the various average values with respect to x and y, always equal by symmetry, we have:

(11) $$\bar{x} = \bar{y} = \sum_k p_k \sum_{i=0}^{k} i \binom{k}{i} \left(\frac{1}{2}\right)^k \qquad (5), (7)$$

$$= \sum_k p_k \frac{k}{2}$$

$$= \frac{\overline{Z}}{2} \qquad (8)$$

$$\bar{x}^2 = \bar{y}^2 = \sum_k p_k \sum_{i=0}^{k} i^2 \binom{k}{i} \left(\frac{1}{2}\right)^k \qquad (5), (7)$$

$$= \sum_k p_k \frac{k^2 + k}{4}$$

$$= \frac{\overline{Z^2} + \overline{Z}}{4} \qquad (8), (9)$$

$$\overline{xy} = \sum_k p_k \sum_{i=0}^{k} i(k - i) \binom{k}{i} \left(\frac{1}{2}\right)^k \qquad (5), (7)$$

$$= \sum_k p_k \left(\frac{k^2}{2} - \frac{k^2 + k}{4}\right)$$

$$= \frac{\overline{Z^2} - \overline{Z}}{4} \qquad (8), (9)$$

$$\sigma_x^2 = \sigma_y^2 = \overline{x^2} - \bar{x}^2$$

$$= \frac{\sigma^2 - \overline{Z}}{4}. \qquad (10), (11), (12)$$

For the correlation coefficient between x and y, we have:

$$r = \frac{\overline{xy} - \bar{x}\bar{y}}{\sigma_x \sigma_y}$$

$$= \frac{\sigma^2 - \overline{Z}}{\sigma^2 + \overline{Z}}. \qquad (10), (11), (13), (14)$$

The Spearman-Brown formula used by Webb and Jones is:

$$r \text{ corrected} = \frac{2r}{1 + r}$$

$$= \frac{\sigma^2 - \overline{Z}}{\sigma^2} \qquad (15)$$

the expression used by Newbold and by Cobb.

II. We say that x and y are independent if and only if

$$p_{ij} = p_{i\cdot} \cdot p_{\cdot j}. \qquad (17)$$

We wish to show that, if z has a Poisson distribution, the distribution obtained by the binomial partitioning is identical with the distribution obtained from Poisson marginal distributions under the hypothesis of independence.

If z has a Poisson distribution so that

$$p_k = \frac{\lambda^k}{k!} e^{-\lambda} \qquad \text{Cramér (3, p. 203)} \qquad (18)$$

we obtain for the joint distribution of x and y by binomial partitioning:

$$p_{ij} = \frac{\lambda^k}{k!} e^{-\lambda} \binom{k}{i} \left(\frac{1}{2}\right)^k \qquad (7), (18) \qquad (19)$$

$$= \frac{\left(\frac{\lambda}{2}\right)^{i+j}}{i!j!} e^{-\lambda}. \qquad (1)$$

Assuming identical Poisson distributions for x and y, we write:

$$(20) \qquad p_{i.} = \frac{\left(\frac{\lambda}{2}\right)^{i}}{i!} e^{-\lambda/2}$$

$$(21) \qquad p_{.j} = \frac{\left(\frac{\lambda}{2}\right)^{j}}{j!} e^{-\lambda/2}.$$

Under the hypothesis of independence, we obtain:

$$(22) \quad p_{ij} = \frac{\left(\frac{\lambda}{2}\right)^{i+j}}{i!j!} e^{-\lambda}. \quad (17), (20), (21)$$

Since (19) and (22) are identical, II has been established. This means that the binomially partitioned distribution introduced by Webb and Jones is identical with one of the cases discussed by the writer (1) in a chi-square procedure. It should be noted that in the special case represented by (18) (Cramér [3, p. 204]),

$$(23) \qquad \sigma^2 = \overline{Z} = \lambda \qquad (8), (9), (10)$$

and

$$(24) \quad r \text{ corrected} = r = 0. \quad (15), (16), (23)$$

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PATTERN ANALYSIS: THE CONFIGURAL APPROACH TO PREDICTIVE MEASUREMENT[1]

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One of the more promising trends in current psychometric research is an increasing concern with methods of evaluating patterns of test scores and test responses. In clinical, vocational, social, and educational psychology, there is a growing consensus that taking account of interrelationships among test variables will contribute appreciably to the efficiency of prediction. Evidence is being offered that higher validities may be obtained if predictive variables are treated as patterns rather than as mere "and-summations" or averages of separate and independent scores.

The purpose of the present paper is to review current theoretical and technical developments in pattern analysis with particular reference to its most recent application, the study of item response patterns within a single test. In this latter instance, our initial hypothesis is that consideration of response configurations will yield more fruitful results than the usual method of reporting merely the total score for a test. Zubin (**28**) pointed out that total scores may conceal as much as they reveal, since the psychological equivalence of the constituent items can seldom be established nor their additive character proved. A total score may thus carry considerably less diagnostic significance than a direct and detailed analysis of the test responses *per se*.

In the present review, attention will be devoted first to considerations emerging from the study of total-score patterns. Subsequently, the application of these principles to the analysis of response patterns within a single test will be examined. Our basic assumption is that test data may be so treated as to yield a higher degree of predictive utility than that obtainable by the more traditional additive methods.

ASPECTS OF PATTERN ANALYSIS

The problem of pattern analysis has at least three aspects which should be differentiated for purposes of conceptual clarity. These may be designated as (*a*) *pattern representation*, (*b*) *pattern matching*, and (*c*) *pattern prediction*.

The adequate representation of a pattern in numerical terms is in itself a problem which necessitates the prior resolution of several statistical difficulties. The main problem is one of developing indices which can simultaneously depict both the amounts or elevations of the several variables composing the pattern and the interrelationships or overlap among them. Another problem is that of correcting for the errors of measurement characteristic of all psychometric instruments, in order to accurately estimate the "true" profile. This becomes a question of preventing the effects of unreliability from contaminating the statistical

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representations of the profiles studied. Such problems must be solved before one can proceed to the second aspect of pattern analysis—that of pattern matching.

In order to determine the degree of congruence between two patterns, it is again necessary to develop indices which can simultaneously consider both the level and the configuration[2] of the variables employed. The subsequent discussion of attempts to solve this problem will serve to illuminate the inadequacies of existing methods as well as some of the basic theoretical and methodological considerations involved.

The third facet of pattern analysis involves the prediction of complex criteria by the simultaneous use of several variables. This aspect of pattern analytic methodology shares certain features in common with both pattern matching and traditional multiple prediction problems. If the criterion to be predicted is itself formulated in terms of a profile, as is the case in many clinical and sociological studies, then the predictive problem becomes one of ascertaining the degree of similarity between the undiagnosed (predictive) profile and the reference (criterial) profile. However, if the criterion is a single composite index, as is frequently the case in vocational and educational measurement, then the investigator's problem involves ascertaining that amount and configuration of variables which will yield the best prediction of the global criterion involved. *Pattern prediction* may thus include *pattern matching*, and both may depend heavily upon the prior development of adequate methods of *pattern representation*.

[2] Level refers to the amount or intensity of any given trait. Configuration takes into consideration the interrelatedness of the variables.

These distinctions underlie the subsequent discussion of pattern analytic theory and methodology. Any statistical technique which employs several variables and attempts to capitalize upon the predictive power contributed by a consideration of their interrelationships should find a place in the above classification.

Analyzing a Pattern

Cattell (**2**) has pointed out that since a pattern consists of both elements and relations, a duality exists in every pattern which must be considered in its definition. For predictive purposes, however, it is possible to divorce these two aspects of a psychometric profile and to study them either separately or in combination. Thus attention may be concentrated only upon the interrelationships among the elements, it may consider both configuration and level simultaneously, or it may be concerned merely with the levels or elevations of the component variables.

Exclusive concern with either shape or elevation alone ignores the possibility that these two characteristics of a pattern may be psychologically interdependent. Although some writers do not consider both level and configuration essential to maximal discrimination, it has been asserted by Du Mas (**8**), and recently emphasized by Osgood and Suci (**20**), that similar patterns located at different levels may indicate interpretations quite at variance with each other so far as behavior is concerned. As the latter authors point out, "Measurement specialists have often expressed concern, for example, over the fact that a moron and a genius may have closely correlated profiles, despite the absolute discrepancy between their scores" (**20**, p. **251**).

It is thus apparent that although

psychological measurement still depends heavily on level alone for discrimination, there is a growing belief (**2, 5, 8, 14, 19, 28**) that (*a*) configuration is important in its own right, (*b*) it is more than a mere summation of constituent elements, and (*c*) it contributes most to prediction when taken in conjunction with level (i.e., with data on the relative amounts of the profile variables being studied).

These principles have received widespread application in recent vocational and educational research (**11, 12, 25, 26**). In the 1930's, an early attempt to objectify intuitive methods of interpreting aptitude test profiles was made by the Minnesota Employment Stabilization Research Institute (**11, 21**). Batteries of tests were administered to persons in a variety of occupations in an effort to ascertain the nature of their test-score patternings. The findings indicated that individuals successful in a given occupation tend to manifest patterns of ability quite distinct from the patterns exhibited by persons in other occupations and in the general population. Subsequent testing of individuals with the same batteries thus gave vocational guidance a more solid foundation by permitting the individual profiles to be compared for similarity with those previously found to characterize the various occupational groups. This rationale has directed the subsequent construction and use of such standard batteries as the Differential Aptitude Tests and the USES General Aptitude Test Battery. A significant aspect of these developments is that recognition is given to the importance of the configuration as well as to the amount of the separate aptitudes measured.

In clinical psychology, the analogues of pattern are the concepts of *type* and *syndrome*. Although *types* have fallen into disrepute because of their tendency to multiply and their lack of specificity regarding component variables (**25**), this concept might be revived if sufficiently powerful statistical techniques were available.[3]

The *syndrome*, no doubt because of its respectable medical parentage, has retained sufficient popularity to stimulate attempts at statistical refinement. The concept of the syndrome underlies the development of multiscore clinical tests which yield configural results based on several different tests or subtests.

Cronbach (**5**) has recently suggested that the data obtained from multiscore tests (e.g., Rorschach, MMPI, Wechsler-Bellevue) and factorial inventories (Guilford, Thursone) may perhaps best be conceived as points in k-dimensional space, where k equals the total number of traits measured. The study of patterns is then resolved into the attempt to describe how these multiple measurements are distributed in k space and how the distributions compare with one another. From this point of view, the problem of pattern analysis becomes that of identifying regions of score concentration in k space, and determining the significance of the obtained differences in the distances among those scores.

Although the statistical comparison of score profiles is logically justified by the k-space concept, Cronbach emphasizes that the existing methodology of such analysis is still inadequate. A primary difficulty is that of accurately determining the extent to which two patterns agree when the component scores of either or both may be unequally reliable and unequally correlated. Under such conditions, confidence limits for the significance of obtained differ-

[3] Stephenson's (**21, 22**) application of the *Q*-sort technique to Jungian typology has been an attempt in this direction.

ences are difficult to establish. Other problems as yet untouched or inadequately handled include those of curvilinear distributions and multimodal concentrations in *k* space, as well as that of dealing with the changes with time which may occur in any of the dimensions measured.

In summary, the shortcomings of current methods for dealing with the multiple discrimination problems involved in pattern matching include failure to correct for unfulfilled assumptions of (*a*) linearity, (*b*) equality of reliability, and (*c*) equality of intercorrelations among items or tests. In addition, some techniques fail to take account of the direction of obtained differences, or even more important, concentrate on configuration to the *exclusion* of level. However, their contributions to objectivity of interpretation justify brief mention of several representative approaches to this problem.

Comparing Test Patterns

Zubin's similarity coefficient (**28**). This is a relatively inexact method dealing only with dichotomous scores. It is based on the number of variables for which the scores are the same, divided by the total number of scores in the pattern. This technique ignores both elevation and the expected frequency of agreements (**2**).

Du Mas' "slope" method (**7**, **9**, **10**). This quick but rather gross method describes the similarity of profiles in terms of the slopes of corresponding segments. Though taking into account both level and configuration, the method is imprecise and can eventuate in maximally good prediction when the two profiles being compared are actually dissimilar (**8**).

The r_p coefficient of pattern similarity. Developed by Cattell (**2**), this coefficient yields an over-all index of profile relations based on the discrepancies between means for each variable, and thus can be used as a measure of absolute agreement between two patterns. However, it may not take sufficient account of configuration in some cases, and is based on the assumption of uncorrelated and normally distributed components (**2**).

The D coefficient. Cronbach and Gleser (**6**) have developed an index (*D*) of dissimilarity between score sets based on the distances among score coordinates in *k*-dimensional space. Similar to a measure recently developed by Osgood and Suci (**20**), this coefficient has a known distribution and thus can be used for testing the significance of obtained differences between groups or individuals. It is useful even when the variables employed are intercorrelated, but in this case leads to problems of weighting, since all variables are not equally important or general.

The checklist method. Frequently utilized in the study of Rorschach protocols, this technique utilized lists of *signs* characteristic of some special group serving to differentiate it from other groups. The checklist total score essentially represents a multiplication of single-score methods and thus tends to ignore configuration and to lose information when the component variables are intercorrelated. It has the further disadvantage of capitalizing on differences due to accidents of sampling (**5**).

Ratios among the profile variables. The reliability of the variables is of particular importance in the use of this method, since minor score changes may drastically affect the size of the obtained ratios (**4**). Moreover, the technique ignores level and tends to grow cumbersome if the number of variables involved is large (**2**).

Correlation coefficients among the scores. The use of product-moment *r*'s ignores the relative amounts of

the components being compared and for that reason is unable to yield a measure of absolute agreement (congruity) between two patterns (**2**). Although its singular assumption of linearity of relationship may be circumvented by the use of eta coefficients, the method in either case makes it possible to obtain perfect comparison when the two profiles are actually very dissimilar in both level and pattern (**8**). *Q* technique tends to restrict study to those subdivisions of *k*-dimensional space where many cases fall (**5**) and groups patterns according to gross similarities without adequate reference to level (**2**).

Pattern tabulation. Developed by Cronbach (**3**), this method employs normalized scores and profiles expressed in terms of the deviation of such scores from their average for each person or group. The technique can handle only two or three variables at a time, and functions best when the scores utilized are equally reliable and equally correlated (**4**). In using this procedure one must assume that profiles having the same shape are psychologically equivalent, since it ignores level by defining configuration in terms of deviation units.

Multiple regression equations. This technique considers the intercorrelations of scores and weights them appropriately, giving good predictions even when dealing with unreliable figures containing large random elements (**2**, **4**). However, it may prove unstable on cross validation because random variation often falls to the experimenter's advantage by contributing maximally to the weights first obtained. Like the factorial specification equation, which is a more specific expression, multiple regression assumes linearity and takes no account of the possible optimum level of each of the variables (**2**). In addition, the composite index it yields is based on a combination of several variables and consequently throws away much information by reducing several dimensions to one (**5**).

Discriminant function. This formula will discriminate two categories of subjects from a mixed sample and thus is of value in studies comparing different types of subjects (**4**). Like the multiple regression equation, however, it loses information by reducing several dimensions to one composite index. Although it does correct for intercorrelation and unequal reliability of variables, problems of weighting tend to diminish the efficiency of this technique.

Many of the procedures so far developed for the description and comparison of score patterns suffer either from a lack of flexibility or from inability to meet the necessary statistical and psychological assumptions. As pointed out above, improved devices are needed for estimating *true* profiles, for weighting the factors appropriately and for evaluating the significance of obtained differences when samples are compared via orthogonal models on many unequally fallible, correlated measurements. Moreover, statistical treatment frequently requires that the data be reduced to fewer dimensions by considering certain patterns to be equivalent so that samples sufficiently large may be obtained to insure adequate reliability (**5**). But despite their deficiencies, the emphasis on patterning inherent in these techniques represents a much needed trend toward the development of objective means for representing and evaluating the organization of ability and personality traits as they occur both in the individual and the group.

Analysis of Response Patterns

The first explicit recognition of the predictive advantages to be gained from the study of items within a

single test appears to be attributable to Brigham (**1**). In his *Study of Error*, published in 1932, Brigham summed up the results of six years' work with the Scholastic Aptitude Test in the following statement:

> These studies might be summarized by the general conclusion that a test item, regarded as a specific problem situation to which a certain number of answers may be made, when given to two or more populations sampled for study in the same manner, will show approximately the same distribution of criterion scores for each possible answer in the several groups sampled (**1**, p. 69).

Brigham regarded this finding as having great importance for educational research, since it demonstrated that important information may be lost if all persons who fail an item are assigned a score of zero for that item and are thereby treated alike. The results obtained suggested that it is possible to specify patterns of response peculiar to and characteristic of particular groups of people. Brigham thus emphasized the potential fruitfulness for predictive purposes of breaking up the typical datum of educational measurement (i.e., the total score in a series of tests or subtests) into smaller units consisting of particular test items. By studying individual test items as specific situations and by analyzing the patterning of errors, he hoped to isolate specific response configurations which could be used to differentiate various classes or types of individuals from each other and from mixed samples.

Study of the significance of associations among test items was subsequently taken up by Zubin (**27, 28**), who pointed out that the traditional practice of assigning empirical weights to responses and summing these to obtain a total score assumes unjustifiably that numerically equal scores are psychologically equivalent. Zubin contended that a knowledge of the patterns of test responses may be more important than knowledge of the total number correctly passed. He hypothesized that response patterns, or clusters of responses to a given group of items having a frequency greater than that expected by chance, will yield results of greater diagnostic significance than a total score which represents merely the summation of discrete responses to qualitatively different items.

To test this hypothesis, Zubin selected a number of personality inventory items, each of which was believed capable of discriminating between normal and abnormal groups. Combining these items into triads, he tested for the significance of pattern differences by the dichotomous score method outlined above (*Zubin's similarity coefficient*). It was found that scoring by patterns rather than by individual responses is possible, and that normal subjects show significantly more consistency of pattern than psychiatric patients. The patients exhibited several unique patterns characteristic only of their own diagnostic group. There appeared to be differences among the psychiatric categories in the extent to which they displayed normal and unique patterns of response. Although each item that entered into any of the triads was itself diagnostic, a further significant finding was that not all of the triads of these individually discriminatory items proved to be diagnostic as a pattern.

The other side of this coin has recently been considered by Meehl (**19**), who points out that configural scoring of an item pair may give perfect discrimination between two groups even though each item taken separately has zero validity. Thus, dichotomous items having no discriminatory power by themselves might separate two population groups

completely when scored with respect to pattern.

Lubin[4] has appealed to Rao's (**22**) maximum likelihood scoring technique for a general solution to "Meehl's paradox." According to Lubin, Meehl's finding applies to continuous as well as to dichotomous variables, and to the case where both groups are scored on but one variable (differentiation being achieved by means of individual item scores). Rao's method applies to these and to all other problems which involve assigning subjects to one of several categories and computing the proportion of misclassifications to be found. Essentially, it involves reducing the number of misclassifications to a minimum by calculating the likelihood of a particular response pattern for each group and then assigning all subjects with this pattern to the group where the likelihood is maximum.

The studies reviewed suggest that configural analysis as applied to item responses within a single test may yield better discriminations than the more usual additive techniques which ignore inter-item relationships. It is implied in the rationale of such analysis that the joint presence of several responses carries a diagnostic significance not inherent in those responses when taken by themselves. Less explicitly, this concept is inherent in the practice of developing a number of different scoring keys for the same test or inventory on the basis of empirical data showing the relationships of these items to various criteria. The utilization of patterns of response represented by the development of such keys not only yields higher validity coefficients but also makes it possible to use the same psychometric instrument for a variety of predictive tasks (**14**).

These considerations underlie much of the research undertaken by McQuitty (**15, 16, 17**) since his attempt in 1937 to differentiate between normal and psychiatric groups by the use of response patterns. McQuitty's investigations constitute perhaps the most extensive attack made to date on the diagnostic significance of response patterns in personality questionnaires. The most recent development of his technique (**18**) involves ascertaining the degree of association between every response alternative and every other response alternative for all items in the test being studied. The obtained matrix of association coefficients is factor analyzed, and a table giving the factor loading of each response alternative is prepared. "Factor measurements" for a given individual may then be obtained from this table by noting the factor loadings for each of the responses chosen by that individual. From such loading patterns, it becomes possible to ascertain the relative degree of integration manifest in the person's responses.[5]

Thus, if the subject has chosen many response alternatives characteristic of a large percentage of the original group, i.e., many responses having high loadings on the first factor, his pattern may be said to represent majority opinion and his personality said to be integrated according to contemporary cultural standards. If, on the other hand, his responses may be accounted for only by means of a large number of factors, the subject may be regarded as relatively disintegrated, since his response pattern suggests the presence of attributes characteristic of several disparate groups. This interpretation

[4] LUBIN, A. Maximum likelihood scoring: a solution to Meehl's paradox. Unpublished manuscript.

[5] MCQUITTY, L. L. Implications of certain measures of personality integration for theories of social psychology. Unpublished manuscript.

is supported by other evidence indicating that group conformity is highly related to personality integration in the sense that an inventory of items weighted to measure the latter discriminates between community persons and mental hospital patients (18).

The basic hypothesis tested in these studies was that personality is integrated to the extent that the individual's ideas are consistent with each other, and subsequently to the extent that his responses to a personality inventory may be accounted for in terms of a single factor. Through its attempt to heighten discriminatory power by taking into account the interrelations among test items, this method of "attribute analysis" underlines the potentialities inherent in the configural approach to the study of test data. Moreover, its usefulness is not restricted to personality test materials. As applied to ability tests, the significant aspect of the technique is that the relation of each test response, whether correct or incorrect, is computed with reference to every other response.[6] In contrast to the usual total-score approach, the method therefore provides for a new interpretation of wrong responses, on the rationale that the individual does not respond randomly when he does not know the correct answer. That there may be meaningfulness in the individual's pattern of wrong responses or choice of plausible distractors is an hypothesis which attribute-analysis is eminently suited to test.

[6] Gaier, Lee, and McQuitty (13) have recently utilized attribute-analysis in an attempt to isolate patterns of reasoning from response interrelationships characterizing a test of logical inference. Five interpretable factors, tentatively identified as distinct response sets or patterns of response, emerged from a 120-variable factor analysis (i.e., intercorrelations of 24 items, each with five possible answers). The findings suggest that attempts to capitalize upon the consistency manifest in response patterns may considerably augment the amount of information obtainable from predictive instruments.

Summary

The studies reviewed indicate that considerable predictive advantage is to be gained by giving adequate statistical recognition to the configural aspects of test scores and reponses. Research to date has demonstrated that the discriminatory power of a prognostic instrument is partially a function of how it is scored, and that an analysis which takes into account patterns of response may have more predictive usefulness than the traditional total-score approach. The questions at issue here are but a special case of one of psychology's general problems, i.e., that of describing the relative frequencies of response in each of the classes it has abstracted from the behavior complex. Recent theory has questioned the utility of traditional additive techniques and has focused attention on the analysis of patterns and dependent probabilities among both tests and test responses. The trend of the evidence suggests that this new direction of emphasis will in the future be to the decided advantage of the predictive endeavor.

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EDITORIAL NOTE: AREA REVIEWS AND MULTIPLE REVIEWS

As an experimental innovation, it has been decided to evaluate newly published textbooks in a given area by grouping them instead of following the traditional procedure of having a separate review for each individual book. Such area reviews will appear as SPECIAL REVIEWS. The review of *Some Recent Textbooks in Social Psychology* by M. Brewster Smith in this issue is the first attempt in this direction.

This issue also includes an example of multiple reviewing. Victor's book on handwriting is reviewed both by Werner Wolff and by Julian Rotter. It was felt that since some psychologists would react negatively and some would react positively to this book, an attempt should be made to have different kinds of reaction presented. In a controversial field, perhaps fair treatment can best be assured if more than one point of view is represented. It is planned that area reviews and multiple reviews will be given further trials.

W. D.
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PSYCHOLOGICAL BULLETIN
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SPECIAL REVIEW
SOME RECENT TEXTS IN SOCIAL PSYCHOLOGY

M. BREWSTER SMITH
New York City

ASCH, SOLOMON E. *Social psychology.* New York: Prentice-Hall, 1952. Pp. xvi+646. $5.50.

DOOB, LEONARD W. *Social psychology. An analysis of human behavior.* New York: Holt, 1952. Pp. xix+583. $5.00.

FARIS, ROBERT E. L. *Social psychology.* New York: Ronald, 1952. Pp. vii+420. $5.00.

HARTLEY, EUGENE L., & HARTLEY, RUTH E. *Fundamentals of social psychology.* New York: Knopf, 1952. Pp. xix+740. $5.50.

SWANSON, GUY E., NEWCOMB, THEODORE M., & HARTLEY, EUGENE L. *Readings in social psychology.* (Rev. Ed.) New York: Holt, 1952. Pp. xix+680. $5.00.

Social psychology, as its composite name suggests, is a marginal field. It is well known that the first two texts to bear the title came forth in the same year from a psychologist, McDougall (**6**), and a sociologist, Ross (**10**). The dialectic interplay between the psychological and sociological approaches that runs as a persistent theme through the subsequent development of the discipline is still with us, as the current crop of texts attests.

Over the years, social psychology seems rather to have suffered the handicaps of marginality than to have reaped its benefits: the characteristic difficulty of the marginal in establishing self-identity has been balanced only recently if at all by the creative ferment that marginality sometimes releases. Perhaps, it might be argued, the trouble was that the parent fields were too immature at the time they conceived social psychology to pose the right problems for their hopeful offspring to resolve: the "developmental tasks" may have been wrongly selected and paced. Or the methodological poverty of its family background may have left the infant science ill-equipped to make its way in the world of complex affairs that everyone agreed was its proper domain. At all events, the rapid growth and transformation of social psychology has come only in the recent postwar years. All of the books under review here bear the impress of these recent developments, though together they come to no resolution on the identity problem and they vary in the extent to which they give promise of emerging synthesis.

A further characteristic of social psychology was foreshadowed by Ross and McDougall: the landmarks in the development of the field have been its textbooks. It is doubtless a sign of persisting immaturity that the basic textbook with its necessarily limited scope still recommends itself to competent social psychologists as a vehicle for their major theoretical contributions. Yet most of the authors of the present texts have ambitions beyond the production of useful adjuncts to teaching, and one of them—Asch—has written a work of major theoretical significance.

Directions in Contemporary Texts: A Bird's-Eye View

It is the lack of consensus on the

boundaries and organizing conceptions of social psychology, of course, that challenges the textbook writer and makes him influential. This lack of consensus is amply reflected in our four 1952 texts, which virtually box the compass of current perspectives on the field. They are not, to be sure, located at cardinal points on pure dimensions, being pitched at different levels and blended with varying amounts of eclecticism. Taken together, however, they afford a good opportunity to get one's bearings on social psychology, not only as it is being taught to the present generation of undergraduates but also as it is giving direction to the ever-increasing stream of research activity.

Faris represents one direction in almost pure type: the line of interactionist social psychology out of Mead and Cooley. Faris is a sociologist, and to find a psychologist in the lineal ancestry of this sociological tradition one must go back to John Dewey and the birth of functionalism. Faris does not neglect the work of psychologists since Dewey, but neither does he draw much that is consequential from them. Mostly he views with critical alarm; besides that, he selects from the psychological literature to embroider and document his account. Another recent text in the same tradition is that by Lindesmith and Strauss (**5**).

Between them, the Hartleys and Doob fill in the region in which the center of gravity of psychological social psychology surely lies. The Hartleys represent the wing that has been more exposed to sociological thinking of the sort drawn on by Faris. For them, communication is the central social-psychological process and socialization the principal developmental focus. Role, status, and social norms provide the conceptual armory for analyzing the individual's contemporary relations with groups. As for their assumptions about underlying psychological processes, they are eclectic and not particularly rigorous, drawing on learning research with no commitment about the nature of the learning process, placing considerable emphasis on perception, and making free use of the amalgam of psychoanalytic and anthropological thinking that has wide currency in discussions of culture and personality. The reader will recognize that this book belongs to a thriving genus of which recent texts by Newcomb (**8**) and Sargent (**11**) are also members.

Of the books under consideration, Doob's makes the least pretension to theoretical synthesis. Aimed squarely at the college sophomore, it shares with Britt's still recent book (**2**) a wide-ranging concern with interesting social phenomena without much emphasis on abstract theory. The perspective, all the same, is one of S-R reinforcement doctrine. While Doob wrestles with the claims of individualistic vs. group-centered approaches—a dichotomy that leads him into difficulties from which the other three interaction-centered texts are relatively free—there is little doubt that his sympathies lie in the individualistic direction. Since he disavows systematic objectives, one must still turn to Miller and Dollard (**7**) or to F. H. Allport's classic (**1**) for clear examples of American behaviorism in social psychology.

But it is Asch who has provided us with another classic to set against F. H. Allport. Writing from the position of Gestalt theory, Asch takes a stand far enough from the center of gravity to wield an Archimedean lever on the world of contemporary social psychology. While some of his strictures against the dominant psy-

chological trends resemble those made by Faris from his sociological bailiwick, Asch's analysis cuts much more deeply and shows surer discrimination. His closely reasoned examination of underlying psychological assumptions warrants the closest attention from the majority of American psychologists who will find themselves in frequent disagreement.

As for its positive emphases, the book does not attempt systematic coverage, however systematic its perspective. The Gestalt emphasis on cognitive processes informs his original treatment of interaction, and throws into prominence a variety of unorthodox problems on which his own considerable research contributions are presented among others. Of recent texts in social psychology, that by Krech and Crutchfield (**4**) resembles Asch most closely in orientation. Krech and Crutchfield write the more systematic text, but Asch's work is more searching and far-reaching in its implications.

Now for a closer look at these four texts, and at the revised book of readings by Swanson, Newcomb, and Hartley that samples so well the research literature on which they draw.

Faris

So far as its likely use in departments of psychology is concerned, the Faris book could be summarily dismissed. It deserves to be taken more seriously, however, since it is written by a competent sociologist and will undoubtedly be used in many of the social psychology courses taught in sociology departments.

The very first sentence establishes the tone of controversy against "individual" psychology that pervades the book:

> The importance of social psychology in contrast to individual psychology lies in the fact that the complex behavior of daily life is not fully explained by an inventory of the physiological system of man (p. 3).

It is the scarcely tacit assumption that the "individual" psychologists of psychology departments are mere psychophysiologists, who have left it to the sociological social psychologist to develop the only psychology with human relevance. It follows, then, that the author's task is to teach all the psychology his students will need. Better that they should not have had courses in general psychology, for if they have, there is much to unlearn. Accordingly, over half of the book is devoted to his own general introduction to psychology, under such chapter headings as "Inadequacy of Biological Motivation," "The Emergence of Consciousness," "The Concept of the Unconscious Mind," "Social Determination of Learning, Perception, and Memory," and "The Social Factor in Ability."

In keeping with his definition of the field of social psychology as "the organization and control of behavior in primary interaction" (p. 4), he then turns to primary group processes and role differentiation and conflict. Aside from a final cursory chapter on "Trends and Problems in Social Psychology," the remaining chapters on "The Neurotic Role" and "Personality Disorganization" would normally be treated in other courses by psychologists.

Since so much of the book is devoted to rewriting general psychology, the content of Faris' revision warrants attention. While there is a liberal sampling of citations from recent psychological literature, one's confidence in his judgment is weakened by the extent to which he leans

on secondary sources, particularly the standard elementary text edited by Boring, Langfeld, and Weld, from which he repeatedly quotes excerpts under the scarcely disarming phrase, "As Boring writes . . . " (see, for example, p. 88). Then, too, one who has graded tests in elementary psychology shivers a little to read of the Rohrschach (sic) test (p. 386).

There is no space to detail the many points in which psychologists are likely to regard his treatment as less than satisfactory. Let me simply list a few:

1. He espouses an extreme environmentalism, even in respect to the sources of exceptional abilities (p. 234).
2. His criticism of conditioning takes stimulus-substitution for granted, and completely by-passes Hull to attack old Pavlov (pp. 78–85).
3. In his extended exposition of a functionalist interpretation of consciousness he presents the still controversial motor theory as established fact (p. 95).
4. In his attack on physiological drives as a basis of motivation, he ignores the conventional distinction between drive and motive, and hence bests a straw man (pp. 18–23).
5. He spends a chapter on an irrelevant refutation of the "unconscious mind," in the course of which he rejects the concept of repression on such flimsy and ill-conceived grounds as these: repression is unnecessary for the explanation of forgetting (p. 122); subjects with hypnotic or hysterical anaesthesias can be tricked into making discriminations within the supposedly anaesthetized range (pp. 132–139); the associative imagery in Coleridge's *Kubla Khan* can be traced to sources in his previous reading (pp. 141–144).
6. His interpretation of the neurotic role solely in terms of secondary gain leads him to embrace what is implicitly a smug moralism. Thus, he speaks of a combat psychiatric case in these terms: "When he was brought wounded to the hospital he woke up shaking and nervous —a condition which could be maintained as a *comfortable* method of escape from further combat and mental conflict" (p. 320, italics mine).
7. He introduces the sociogram as "a device somewhat similar to a scale" (p. 402).

Apart from his inadequate command of contemporary psychology, and his inadequate criticism of its vulnerable points, there is of course great merit in the interactionist position as he expounds it. And it is true enough that psychologists have too often given lip-service to the interactive sources of human nature in society without carrying out the radical revision of their concepts that such a position entails. It is also unfortunately true, however, that the interactionist theory which burst full-blown from the pens of Cooley and Mead, having generated singularly little research, remains in Faris' text essentially as its brilliant authors left it. Surely the most careful and ingenious research will be required to verify and refine the notions of role taking and empathy and self that remain fixed points in this sociological current. But the perfunctory roll-call of research techniques in his final chapter leaves Faris firmly entrenched in the discursive rather than experimental tradition.

The Hartleys

Instructors who want a fully packed and up-to-date text that acquaints the student with current thinking and research in the areas most frequently regarded as social psychological will probably prefer this book to the others in the present list. It is a fat book, replete not only with references to the experimental literature but with clinical case summaries and anthropological accounts. And it covers much ground judicious-

lly. The chapter on attitudes to which Clyde Hart has contributed is probably the best brief statement available on current research and theory on the topic.

Yet for all these strong points, the Hartleys' text is not fully satisfactory. It is loosely organized, the chapters being divided into many brief sections among which it is not hard to lose the thread. The major parts of the book, on communication, socialization, and group processes, are only very tenuously bound together, in spite of some repetitiveness. The treatment of underlying psychological processes is rather loose and uncritical. And the considerable number of misprints and mistakes in bibliographical citations bespeaks hasty editing. Again I shall list my more specific criticisms for the sake of brevity:

1. The essay by Gerhart Wiebe on "Mass Communications," included by the Hartleys in keeping with their practice of concluding each major part with an applied chapter, is special pleading. Without questioning Wiebe's data or the soundness of his arguments, one notes that he *is* arguing in defense of the commercial media, rather than examining dispassionately just what their impact may be.

2. "Self-interest" is proposed as the principal determinant of set (p. 42), though even without the benefit of Asch's trenchant criticism this hoary and ambiguous mainstay of popular psychologies would seem patently inadequate to the complexities of human motivation. To break it down into "learning and yearning," as the authors do later (p. 133), does not help.

3. Part II, on socialization, does not treat the learning of roles, a topic relegated to the third part. While this allocation may make for convenience of exposition, it is at the cost of theoretical integration.

4. The treatment of social norms suffers from the usual ambiguity between mere *uniformities* of social behavior and standards of *evaluation* that carry the compulsion of legitimacy. The chapter on "The Functioning of Social Norms" shows particularly poor organization, with dangling sections on intergroup relations and group productivity the relevance of which is unclear. In the latter section Thelen's list of principles for "change agents" is simply appended without discussion.

5. Examples are continually drawn from the field of prejudice and intergroup relations, one to which the Hartleys have of course made a notable contribution. But the net impact on the reader may convey a rather specialized notion of the preoccupations of social psychology.

6. In drawing eclectically on many sources, the authors sometimes sacrifice theoretical rigor. They adopt, for example, a stimulus-substitution diagram of the deconditioning of children's fears (p. 248). They reprint the list of devices promulgated by the Institute of Propaganda Analysis (p. 85) almost without discussion, in spite of the dubious psychological value of this classification, and the still more dubious prospect of communicating much to the student from a mere list.

7. Occasionally the authors' interpretations of research data seem bizarre to me. Thus the increase in intelligence test scores of Negro children after migration to the North is attributed to group pressures toward conformity (p. 427). For another example, the fact that children conceive of their parents as embodying multiple roles successively rather than simultaneously is interpreted as a sign of their restricted experience and intellectual limitations (p. 527), when the artificial abstractness of the assumed adult conception would seem the more remarkable.

The book remains a useful one, though it would be a better text if it were less inclusive, and more substantial theoretically if it were more tightly woven.

Doob

It is hardly fair to criticize Doob's book on points of theory, since it is so clearly designed as a text for a quite elementary course. In its own terms it succeeds very well indeed. Its marked virtues follow from Doob's restraint in setting himself to produce a textbook, not a research manual or a new theoretical synthesis. As a novel feature, a large number of brief and well-edited excerpts from original sources are interwoven in the text—descriptions of experiments, writings of classical theorists, sociological descriptions, even historical accounts and literary passages. These are admirably selected to attract student interest and provoke student thought. If the book is light on abstraction and systematization, it shows more than usual pedagogical skill in inculcating scientific modes of thinking about social behavior.

While I would therefore rank the book high among those of its level of difficulty and degree of popular interest, I must take serious issue with the underlying assumptions that structure his approach to the field. In a summary passage he states:

> The orientation to the group can be on the level of the individual: other people may be viewed simply as additional stimuli. *But* it is so very difficult to apply those principles simultaneously to many individuals . . . . In addition, people in groups may behave so differently that a new set of principles on an entirely different level—the level of the group—seems necessary . . . . The protagonists in this controversy do not seem to be reaching agreement . . . . Maybe social psychology must function for the time being on both levels: let the group-orientation discover and pose new problems, and let the individual orientation attempt to solve them by supplying the details and even the principles (pp. 554–555).

But the protagonists in the controversy are reaching more agreement than Doob believes—or, more accurately, they are gradually being replaced by others who have been reared to a definition of the situation that makes Doob's dilemma obsolete. It is unnecessary to remain trapped in the dull historic controversy of individualism vs. the Group Mind. Asch makes the most incisive analysis of how this false dichotomy arises from a failure to examine the psychological character of interaction. But even without Asch, the account of interaction that Hartley and other writers in the same vein have modified from the sociologists at least indicates the direction from which resolution is to be sought.

Doob's position has major consequences for the subject matter of his book. The relational variable of role, for example, is completely ignored, while social class, which can be regarded as either a group-level concept or a *property* of individuals, is given extensive treatment. Lacking the interactional conception, it seems fair to say, social psychology has no boundaries for Doob other than those of interest. There is no theoretical criterion for inclusion or exclusion. And, accordingly, one finds such miscellaneous items as a long—and and educationally sound—treatment of the physical anthropology of race (pp. 161–183), and chapters on vogues, social conflicts, and the diffusion of change.

A word about the tone in which the book is cast. It seems to me both unnecessary and regrettable that Doob has often seen fit to fish for student sympathy and interest by self-deprecating remarks, flippancy, and the j cy tidbit. Doob's style is

otherwise sufficiently attractive and the content of enough intrinsic interest that these classroom tricks only detract from his otherwise laudable attempt to convey the scientific spirit.

Asch

If Doob writes for the sophomore, Asch writes for his professional colleagues, the graduate student, and advanced undergraduates who are ready to find excitement in a venture in critical and constructive thought. There are other books from which the student will learn more of the content of contemporary social psychology, but none that exposes the reader to so searching an examination of its assumptions and their consequences. A book that has obviously been years in the writing, it will be read for a long time to come. And its clear but often quietly eloquent style makes the reading a more than intellectual pleasure. Asch writes a thoroughly psychological social psychology that makes no claims to empire in the social sciences:

> The study of social behavior is part of the task of general psychology. Its facts and principles cannot be derived from the study of behavior outside the social setting. At the same time a theory of society cannot be exclusively psychological. The interrelations among the actions of the members of society reveal regularities and tendencies that can be studied in their own right; these are the province of the social disciplines (p. 38).

If social psychology must build on its own distinctive facts and cannot perform its function either by the rote application of principles derived from asocial contexts or by beachcombing operations in other social sciences, as Asch's contention has it, it has not got very far on its task.

> It has to be admitted that social psychology today lives in the shadow of great doctrines of man that were formulated long before it appeared and that it has borrowed its leading ideas from neighboring regions of scientific thought and from the social philosophies of the modern period. It is paradoxical but true that social psychology has thus far made the least contribution to the questions that are its special concern and that it has as yet not significantly affected the conceptions it has borrowed (p. viii).

So Asch begins by examining some of these "doctrines of man," taking issue with contemporary themes in social psychology to which they give rise. He singles out the following as prevailing dogmas with which he is in disagreement:

> The ego-centered character of men.
> The supremacy of irrational emotions.
> The primacy of rationalization in human thinking.
> The basis of human experience in arbitrary association and conditioning.
> The roots of adult attitudes in childhood experiences.

There is indeed no doubt that Asch is a deviant. Of the two theoretical currents that he identifies as most influential in social psychology, he rejects S-R reinforcement theory outright on standard Gestalt grounds. Psychoanalysis he finds wanting for its omissions and emphases rather than for being flatly wrong: "What is lacking in Freud's account," he says, "is the sense that society is the condition of freedom as well as a source of oppression" (p. 347). Oddly enough, he takes virtually no explicit notice of the additional current of sociological interactionist theorizing, though its principal contentions are fully embedded in his own thinking.

After an introductory exposition of the Gestalt position, he analyzes the problem of interaction from a cognitive standpoint. Would that

the lectures of G. H. Mead had been rendered with such lucidity!

> The paramount fact about social interaction is that the participants stand on common ground, that they turn *toward one another*, that their acts interpenetrate and therefore regulate each other . . . . It is individuals with this particular capacity to turn toward one another who in concrete action validate and consolidate in each a *mutually shared field*, one that includes both the surroundings and one another's psychological properties as the objective sphere of action. . . . The interpenetration of viewpoints once and for all separates social action from acts of individuals which happen in combination to produce certain regular results. The process is not one in which individuals combine like gases to lose their identity and produce something different from either of them. Rather it requires that each participant retain his perspective and assert his individuality (pp. 160–162, italics his).

The most fundamental problem of social psychology, therefore, is the process by which veridical social perception is achieved. Unlike the Meadian theorists, Asch has of course himself tackled this interactional problem with considerable experimental ingenuity. The remainder of this section of the book is "An Introduction to Group Theory," in which, it seems to me, he effectively exorcises the spectres of extreme individualism and the group mind.

The ensuing section on social needs includes, among other things, a critical scrutiny of the doctrine of self-interest, a brief but provocative phenomenological analysis of rules and values, and a sophisticated critique of cultural relativism. The final section, "Effects of Group Conditions on Judgments and Attitudes," draws on his previously published examination of the doctrine of suggestion and contains a report on his recent experiments on the modification of judgments by groups. A brief chapter then takes sympathetic but critical notice of research on small groups in the Lewinian tradition. Two chapters on opinions, attitudes, and sentiments take account of the more quantitative methods of investigation, but rightly insist that the phenomena need to be examined from a more psychological perspective. The final short chapter on propaganda examines recurrent features of exploitative propaganda, and emphasizes its limitations.

As a systematic examination of central problems of social psychology in the light of Gestalt theory, the book is an admirable performance. Its balance nevertheless suffers somewhat from the isolation of American Gestalt psychology and from Asch's almost religious dedication to rationality. Speaking of the reasons for a re-examination of psychological axioms, Asch writes that "there is also a human reason for undertaking [it]: if the grim picture psychology draws were correct, there would be no hope for man or society" (p. 30). Drawing the picture as sharply as he does, perhaps he is right. At all events his attempt to make tacit assumptions sharply explicit, if it does not always result in a fair or flattering portrait of non-Gestalt psychology, creates the conditions for theoretical advance. It demonstrates once and for all, moreover, the weakness of social psychologies that claim neutrality as to the nature of underlying psychological processes. But the fervor that motivates his criticism does make for a one-sided picture. In a scrupulous attempt to be just, Asch leaves the dark side of man's nature to psychoanalysis: he wisely abstains from trying to refute Freud on his own ground. The facts remain, however, that irrational proc-

esses exist, and that they remain residual to his system.

Since Asch makes no attempt to cover in full the conventional ground of social psychology, one cannot criticize him for obvious omissions. It is relevant, however, to point to a a major gap in his Gestalt doctrine of social interaction. It is a crucial assumption of his theory that bodily expressions of inner states mirror isomorphically the psychological processes that they accompany. Asch's brief discussion of this point not merely lacks convincing evidence, but fails to show how such postulated isomorphism can be given intelligible meaning.

Swanson, Newcomb, and Hartley

Since the book of readings edited by Newcomb and Hartley (**9**) has been used so widely during the past five years, the revised edition should receive a warm welcome. The number of separate pieces has been reduced from 81 to 65, of which between a third and a half are new with this edition. Simply to list a few of the newcomers highlights the far-reaching developments that make the initial edition out of date: Asch on the effects of group pressure, Leavitt on communication patterns, Festinger, Thibaut, and Back on interpersonal communication, Bales on interaction process analysis, McClelland and Friedman on cross-cultural research on need achievement, Merton and Kitt on reference group theory, Deutsch and Collins on interracial housing—the array is impressive. As before, some of the pieces were specially written or revised for this volume, a few are excerpted from books, but most are reprinted from the periodical literature. To this reviewer, the arrangement is an improvement over the earlier version, though in a collection of readings order is inconsequential.

In a field so active as social psychology has recently been, hardly anyone will quarrel with the editors' decision to draw heavily on new material. Individual opinion will differ on the wisdom of specific inclusions and omissions, but most social psychologists should applaud the total offering. The book provides ample supplementary reading for all of the texts reviewed here except Faris, and even for sociological courses on his pattern there are a number of directly relevant selections.

Instructors would do well to advise their libraries not to discard their battered copies of the original edition, as the half of the old volume that has not been reprinted contains much material that still is useful.

Some Comments and Speculations

Perhaps it is unduly rash to attempt any general comments on the heterogeneous company of texts we have passed in review. I should like to close, however, with two general remarks: one about the teaching of social psychology and the other about trends in theory.

The books reflect no consensus on the level at which social psychology should be taught or on how much general psychology should be presupposed. The common practice would seem to be to write into a social psychology text all the general psychology that the student presumably needs for the purpose at hand. Often this appears to be very little. In following this custom, our textbook writers may be bowing to realistic necessity, but few teachers would agree, I think, that the situation is satisfactory. Doubtless there will continue to be a legitimate demand for books at a variety of levels

of difficulty. It is of some interest to note, however, that Buxton, Cofer, and their collaborators (**3**) in their recommendations for a revised undergraduate curriculum in psychology suggest that social psychology be placed at the advanced level in their three-step hierarchy. If their proposals should be widely adopted, a different and better pattern of textbook writing would be required.

Despite the seeming theoretical chaos to which these books bear witness, I would venture that the old tug-of-war between sociological and psychological conceptions of the field is undergoing a change, one that will leave stranded those who are too fond of the old controversies. Three of the four texts center their theory around a conception of interaction. Once this is accepted in its full consequences, the causes for battle vanish, though there remain important special considerations for the psychologist and the sociologist who approach the common ground from different directions. As the recent paper by Sears (**12**) gives promise, reinforcement as well as cognitive theory can be given an interactionist phrasing. The alternative to this focus, and I think a mistaken one, is to resign oneself to confusion about the contents of a field with no organizing principle and no boundaries except those set by personal preference.

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METTLER, F. A. (Ed.) (The Columbia Greystone Associates, Second Group.) *Psychosurgical problems.* New York: Blakiston, 1952. Pp. xii+357. $7.00.

This book is a sequel to the one produced by the original Columbia Greystone group, titled "Selective Partial Ablation of the Frontal Cortex," 1949 (cf. *Psychological Bulletin*, 1951, **48**, 185 ff.). It has a number of avowed purposes: (*a*) to compare the less commonly used psychosurgical procedures involving cortical venous ligation, thermocoagulation, thalamotomy and transorbital lobotomy; (*b*) to explore the value of new psychometric tests in assessing clinical status; (*c*) to evaluate the biological effects of transorbital lobotomy; (*d*) to report the status of patients, studied in the first project, two years or more after their operation; and (*e*) to find out why psychosurgery was followed in the first project by recovery in 20 per cent more of the cases than for a control group. Similarly to the first project, the second one is the product of many hands and many different disciplines. The administrative burden of coordinating so diverse an effort must have been considerable.

This second study was beset by many problems. The criteria for the selection of patients were so stringent that, even after some relaxation, disconcertingly small numbers of cases remained in the critical groups. There were 27 operatees and 6 controls in this study. Of the 27 operatees, venous ligations were performed on 12, transorbital lobotomies on 9, thalamotomies on 2, thermocoagulation on 2, topectomy after lobotomy on 1, and enlargement of cortical ablation on 1. All of the 33 cases were schizophrenics.

Not only was the control group small, but for reasons apparently beyond the control of the study, it was poorly matched with the operated group. Advance prognostic ratings on a four-point scale were provided for 31. The average rating for the 6 control cases was 1.8 and for the 25 operatees 2.8. The control group, then, initially appeared to have a better chance for recovery. In addition, the IQ of the operated group was 80.8 with a range of 55 to 114, whereas the IQ of the control group was 100.3 with a range of 83 to 115. The incidence of recovery was small for the entire group; only two of the operatees and one of the controls was considered well enough to warrant parole.

Extensive batteries of psychometric tests were administered at four different times, two preoperative, approximately 60 days and 30 days before the operation, and two postoperative, ranging from ten to 180 days after. The tests included the Wechsler-Bellevue, the Porteus Maze, the Weigl sorting test and tests of incidental memory, verbal directions, shifting set, vestibular function, and olfaction, among many others. During the period covered by these examinations little or no amelioration in mental illness was evident. Performances on the psychometric examinations in this study bear no identifiable relationship to ultimate clinical status.

In spite of these and other problems working against the effort to reduce the ambiguities of the results, the study has considerable value. To begin with, the activities and findings

of all the disciplines are carefully documented. Many new tests were tried and some of them provide promising leads for further development. Even with the small number of cases involved, the evidence is quite convincing that the various psychosurgical insults immediately impair many psychometric and psychophysiological performances. However, with very few exceptions, the manifestations of impairment disappear with time. Among the exceptions to this trend were the inability to benefit from retesting on the Wechsler-Bellevue performance scale and on the Porteus Maze test.

These exceptions are so few in comparison to the number of tests used as to lead the authors to generalize that *all* the psychological test performance changes resulting from psychosurgery are nonspecific and transitory (p. 315). While such a conclusion stretches by only a little the description of the findings of the study, the magnitude of the testing effort may encourage many to believe that the psychosurgical procedures are without some irretrievable cost to the personality organization and performance capabilities of the patient. It is interesting to point out that this question was neither asked by the investigators nor was a suitable experimental design incorporated into the study to provide a definitive answer. While the authors usually take the conservative position that their study merely provides no evidence for such a cost, they are confronted with the paradox that such losses have been reported elsewhere for patients with frontal lobe lesions uncomplicated by psychosis. Note the following quotation:

> The finding of no permanent loss in intelligence resulting from psychosurgery of these varieties has been confirmed. Indeed the puzzling question at the present instant is why nonspecific injury, such as gunshot wounds, pathology of the frontal lobes (neoplasms) or translateral frontal lobotomy (involving the agranular cortex) is frequently attended by a permanent loss of some part of intellectual efficiency as evidenced by intelligence tests. No simple or apparent explanation of this discrepancy is available (p. 314).

The reviewer believes that the paradox is explicable in terms to which the investigators make only passing reference. They recognize that the preoperative test performances of the operatees and the initial test performances of the control group were handicapped by psychosis. It will be recalled that the average IQ of the operated group before psychosurgery was 80.8, the range 55 to 114. The scores of the operatees before psychosurgery appear to be unduly depressed. The authors, in another context, take cognizance of such a possibility in the statement:

> . . . it is apparent that the changes due to recovery from psychosis are mainly if not entirely in the realm of affective attitudes, loss of anxiety, decrease in complaints and the like. *These affective attitudes probably interfere with the quality and quantity of performance in most of the psychologic tests that have been employed in both this and the previous study* . . . (p. 278, italics mine).

They then justify ignoring this important fact in the evaluation of their findings by continuing with the statement:

> . . . but that interference has affected the operated and control groups alike (p. 278).

To be sure, the interference *initially* may have affected the operated and control groups alike, but did they affect them equally *after* the operation? We may suspect that for many pa-

tients the surgical procedures minimized the influence of "interfering affective attitudes" of the psychosis in greater or lesser degree. This effect could well obscure performance impairment expected from brain damage and reported in cases uncomplicated by functional psychoses. For these reasons, the finding that the varieties of psychosurgery used in this study resulted in "no permanent loss in intelligence" may be true only for a schizophrenic population, and at that for one with the age and institutionalization characteristics closely resembling the one studied.

Of course, the foregoing considerations are speculative, but in view of the conflict between these Greystone findings and those contained in a substantial literature on frontal lobe damage it would be premature to conclude from the present study that the psychosurgical procedures utilized will not permanently impair psychological performances. The reviewer fears that the very considerable amount of effort which has been invested in this study will lend weight to the premature conclusion and encourage the extension of psychosurgical procedures to a wide variety of psychiatric categories on the unestablished premise that they can do no permanent harm.

JOSEPH E. BARMACK.
College of the City of New York.

WOLFF, CHARLOTTE. *The hand in psychological diagnosis.* New York: Philosophical Library, 1952. Pp. xv+218. $7.50.

The clinician who approaches this work with the anticipation that he will find a careful, objective analysis of diagnostic indicators and a scientific evaluation of their predictive limits will be disappointed. Nor will he find its theoretical treatment particularly stimulating since it is based on a vague and outmoded "faculty" psychology. A brief quotation may illustrate: "This use of the forefinger transmits the human faculty of discrimination, which is an essential quality of intelligence." The book unfortunately is "clinical" in the outdated, derogatory sense of the term.

WILLIAM A. HUNT.
Northwestern University.

BERGLER, EDMUND. *The superego.* New York: Grune & Stratton, 1952. Pp. x+367. $6.75.

Writing a review of a psychoanalytic book that purports to be scientific involves pronounced occupational hazards, particularly when the author has posted clear warning signs concerning the unconscious motivations of writers, reviewers, and scientists. However—

In *The Superego,* Bergler summarizes and sharpens his psychoanalytic thinking of the last twenty years and presents numerous wide-ranging and depth-centered theoretical constructs. He valiantly attempts to go even deeper than the usual psychoanalytic probing, and to uncover not merely human defenses and resistances, but what he calls the "defense against a defense."

Beginning with the assumptions that "restrictions emanating from the superego . . . are biological" and that "psychic masochism is the universal human 'trait,'" Dr. Bergler insists that "every neurotic symptom has a five-layer structure, only the unconscious defense against the defense becoming visible. The sequence is: (1) unconscious wish emerges; (2) superego objects (Veto #1); (3) first defense is presented by unconscious ego; (4) superego objects once more (Veto #2); (5) second defense is presented by the unconscious ego, guilt is accepted for the lesser crime, reverberations reach the psychic sur-

face, cloaked in rationalizations. The conflict is unconscious, of infantile origin and repetitive. The end result of the infantile conflict is always oral-masochistic. Every neurosis represents a rescue-attempt from the oral danger" (p. 64).

This five-layer structure is then used by Bergler to explain such human behavior as creative writing, cynicism, hypocrisy, pessimism, optimism, pessimo-optimism, wit, self-pity, silence, talkativism, inhibition, exhibitionism, raconteurism, thisism, thatism, and, at least by implication, kitchen sinkism. That there might possibly be some *conscious* or *one-* or *two*-layer unconscious reasons why people should be or do anything whatever, Bergler does not admit.

Never a man to mince words or qualify a dogmatism, Bergler not only outjuggles the Freudians at their deepest depths, but frequently supplies his own italics: "The end result of the infantile conflict in *every* neurosis is exclusively stabilization on the *oral* rejection level which is the basis of psychic masochism" (p. 47). "*Every* joke, in short, has as its butt the inner conscience" (p. 138). "The *severity of the superego*—the inner torture machine—is *unchangeable* because no method has been, or can be, invented to shorten the maturation time of the human child" (p. 325).

What evidence, it may be asked, does Bergler present to substantiate his numberless hypotheses? As far as could be determined from intensive reading of *The Superego*, no evidence but: (*a*) a handful of fragmentary case illustrations with interpretations clearly slanted by the author; (*b*) several references to novels, plays, poems, and biographies, with equally controversial interpretations by Bergler; (*c*) some charming but dubiously relevant anecdotes; and (*d*) several direct and implicational appeals to the authority of Sigmund Freud. About Bergler's hypotheses two more alternate hypotheses may be offered: (*a*) that they constitute brilliant insights into the innermost reaches of the human personality; or (*b*) that they do not. If there is any scientific evidence to support the first of these alternatives it is not presented in *The Superego*.

ALBERT ELLIS.
New York City.

VICTOR, F. *Handwriting*. Springfield, Ill.: Charles C Thomas, 1952. Pp. xi+149. $4.00.

REVIEWED BY
WERNER WOLFF
Bard College

The fact that since 1947 a major study on handwriting has been published each year by a psychologist indicates that American psychologists become more and more interested in handwriting as a diagnostic tool. Although graphodiagnostics in Europe overshadows any other kind of psychodiagnostics, such as the Rorschach, paper-and-pencil testing, or questionnaire inquiry, graphology found great resistance in the United States. One of the reasons was that it had lent itself too much to a mixture of basic graphological observations with speculative theories on personality.

For the first time Victor makes the important point that basic psychological observation, which he calls "basic graphology," is a science of objective observation and can be independent of all graphological theories which, though interesting for any theory of personality, belong to what he calls "applied psychology." It is the opinion of the present reviewer that the author would have better used the term "applied graph-

ology" instead of "basic graphology," and "theoretical graphology" instead of "applied graphology."

Victor calls handwriting a personality projection, considering the letters as somewhat similar to an inkblot. Although, as the present reviewer observed, letters may become a screen for even a projection of images, handwriting seems to him an expression rather than a projection. As a direct expression of inner motions in outer motions, handwriting is a graphogram, more similar to an electroencephalogram than to a Rorschach inkblot.

Handwriting is the recording of inner psychological movements, of which Victor considers as the basic ones release, tension and output of energy. He compares in a suggestive way these different forms of rhythm with rhythmical forms in poetry.

On one hand, the author makes the point that handwriting as a dynamic movement does not allow any fixed correlation between graphic forms and personality traits. On the other hand, he tries to establish a correlation between handwriting and constitutional types according to Sheldon. The author should have elaborated upon this double aspect. Handwriting is both form and expression, frozen gesture and flowing motion, and it is from the relationship of both that the personal equation results.

Victor's book presents a good condensation of graphological concepts. However, it does not present any new observations and does not emphasize enough the need for graphological experiments and for a systematic correlation between elements of graphic expression and definable temperamental, clinical, and professional entities. The author's justified call for a basic graphology can be solved only if basic graphological elements can be correlated with basic mental and emotional patterns to be formulated by a scoring system. This, ultimately, will make graphollogy the most important psychodiagnostic instrument because the diagnosis by handwriting is the only tool which does not require the client's, or patient's, presence, and the diagnosis deals with the direct personality record itself, without using any foreign material such as inkblots or picture cards. With the proper technique it will take the least effort for a major output.

REVIEWED BY
JULIAN B. ROTTER
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Most serious students of projective methods of personality measurement would at least concede that if factors of handedness, conditions of sampling, and special training are controlled, better than chance relationship may be found between some aspects of handwriting and similar aspects of other motor tasks. They might also agree that handwriting may reveal at a better than chance level such characteristics as the educational attainment or experience of the subject. In some cases markedly individualistic writing may allow for the "diagnosis" of some generalized and significant personality characteristics. Speaking generally, however, American psychologists have refused to accept the thesis that handwriting may be confidently used as an efficient, reliable, or valid method of predicting significant personality variables. Victor sets as his goal for this book to promote " . . . as wide, serious and efficient a use of handwriting analysis in this country as it enjoys abroad" (p. 3).

The author feels that part of the handicap of graphology has been that it has been tied too closely to various conflicting schools of psychology, and he advocates a separation of what is "basic" graphology from "applied" graphology. He sees handwriting as a kind of recorded rhythm, and stresses movement concepts as opposed to a static graphology which deals with signs, the shape of specific letters, etc. The movement of the hand on or above the paper, connectedness, and relationship of letters are of major importance. Handwriting analysis deals for the most part with these characteristics of movement, pressure, direction, and the symbolism of space. Some idea of the nature and implications of this type of analysis may be obtained from the following quotations. In the paragraphs below the author is dealing with the problem of depth rhythm, using an analogy of a water current:

> Soon the stream, and with it the stroke, may become sluggish. Their borders are no longer sharp. The written letters show fringes on one or both sides (Figure 6a), as if written on blotting paper. Just as the water runs slowly through meadows and swamps, without bed or border, the life stream of the writer loses its intensity, its liveliness and "directed energy." Having lost its aim, it becomes wasted.
>
> The river bed may dry up. Although the direction of the river's path is recognizable, the water itself, the energy, is gone (the dry gullies in the summer desert). Handwriting analogous to this simulates strength, its borders insufficiently filled with ink, or even bare of it (Figure 8i).
>
> On its way, the river may find a dam in its path. Similarly, handwriting can be dammed up by sudden stops of the pen which retard the forward movement (tense rhythm). When the dam breaks, however, the suddenly released energy of the river as well as of the emotions will cause untold, unforseeable damage. Heavy, suddenly-stopped strokes always carry in them the danger of a break, of eruption and devastation (p. 47).

In dealing with the problem of direction the author has the following to say:

> Where the writing starts, the mind symbolically or unconsciously experiences the *beginning*, the origin; and origin stands for the Mother. The beginning becomes the past just as soon as the writer leaves it on his way forward. He moves away from the past into the future.
>
> The left side, then, represents the past. "Mother" and "left" become interchangeable. To live, in the Occidental way of thinking, means to keep moving forward, toward action, freedom and independence: we move to the right. But at the right is found the antithesis of the mother, he who represents all these characteristics: Father. The right side, thus, becomes identified with the father. These images, of father and mother, influence everyone's life, and their effect on the writer's personality finds its expression in a greater or lesser tendency to favor the left or right (p. 52).

Applied graphology applies the principles of handwriting analysis to any of several personality theories or problems primarily on the basis of analogy. A sample case of graphological analysis is made, and this is translated into personality descriptions in terms of the constructs of Jung, Freud, Horney, and Sheldon with equal facility. In discussing applied graphology, although Victor defends the graphologist's difficulty in determining sex (which, after all shouldn't be difficult since attitudes toward father and mother should at least be related to sex) he does imply that graphology can contribute to the diagnosis of a great many problems providing the referrer states

clearly the problems which he wishes to be answered. The quotation below illustrates some of the potential range of usefulness of handwriting:

> All questions must be precise; the analyst should know whether he is to search for criminal tendencies, for instance, or for neuroses, psychoses, or whatever. If a doctor wishes to know whether the subject is a "bleeder" (hemophiliac), he must at least supply to the analyst the characteristics of this particular disease, and if the doctor wishes information on the progress of a treatment he must be prepared to give all pertinent facts. Even if the graphologist cannot give a diagnosis, he will be able to point out symptoms that may be conclusive enough to the medical man (p. 92).

As to the verification of all this, the author does not believe that much is to be obtained from "mass experimentation" and even when stating that the relationship of middle, lower, and upper zones should be balanced in a 1:1:1 ratio which would be the proportion of idealism, to judgment and feeling, to instinct, he states that this ratio cannot be expressed in inches but that the analyst's eye must be trained to recognize it in the script. Since the author does not feel that it would be profitable to come to grips with the American tendency to insist upon objectivity, quantification and experimental tests, it is the reviewer's impression that this book will fail in its goal of obtaining wide acceptance of handwriting analysis in this country. It should be said in all fairness to the author, however, that the reviewer's handwriting reveals a veritable bonanza of pathologies and produces considerable aggression in his secretary. For the reviewer, the typewriter was a wonderful invention.

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